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THE PSYCHOLOGICAL REVIEW

THE SCIENCE OF PERSONALITY: NOMOTHETIC OR IDIOGRAPHIC? [1]

SAMUEL J. BECK

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A principal and vigorously debated issue before psychology today concerns method, and the adequacy of the known methods of evaluating findings by any of the approaches to the study of the whole personality. The ferment centers especially around the merits of the "nomothetic" as against the "idiographic" logic. First, let it be noted that, so far as concerns the basic procedures of scientific method, the two methods have everything in common. They both have recourse to observation and to experiment. They analyze and resynthesize data. They draw inferences that follow the usual canons of logic, both inductive and deductive. These are the foundational approaches to scientific method. The two procedures—the nomothetic and the idiographic—do differ, however, and critically. The differences have weighty effect in the kinds of hypotheses that can be formulated concerning human behavior. Such effects can therefore be very important for progress in the study of the human personality, since hypotheses are really the product of scientific imagination. The issue becomes: how liberated is the scientific imagination, always the starting point for new experiment into unknown areas of any particular field?

Historically, this nomothetic-idiographic issue could hardly have become so important in psychology before the present time. The nomothetic method has so far been the traditional one. But interest in personality, as personality, is shaking the logic on which it rests. The logic is not proving adequate for psychologists who are focusing on this behavior unit at the same time that they want to adhere to a good method. They have now been hard put to it to find a logic applicable to the phenomenon which they have elected to study.

Some light on the inadequacy of the nomothetic procedure in the field of personality is obtainable by considering the questions that we want answered in such investigations. We may inquire how much of some one personality trait is found in each member of a population under study—bravery, for example, or pride, or sense of humor. Or we may ask about any person how much bravery does he have, *and* coolness, *and* pride, *and* sense of humor, *and* other variables that fuse into character. In the one, we ask what are the incidence and the

[1] Presidential address of the Division of Clinical and Abnormal Psychology, read at the Amer. Psychol. Ass., Washington, D. C., September, 1952.

The thinking in the present paper grew in part out of two researches in schizophrenia (MH 63 and MH 64) conducted under a National Institute of Mental Health grant, in the Psychosomatic and Psychiatric Institute, Michael Reese Hospital, Chicago, Illinois.

distribution of some one datum within a population. In the other, we focus on the various behaviors within one person, their mutual interaction, and their effects in bringing about the total behavior which we identify as a particular personality.

The first of these two ways has so far been the proven one of science. It came to us following long experience in measurement, successful prediction, with resulting control. It has been so successful that it has captured the interest of all scientists, and that is equivalent to saying that it has harnessed the scientists themselves. To harness is to bind. It is a circumstance which can have disadvantages, restrictions. This is the situation which dominated in the natural sciences for a time, then in the social sciences, and finally in psychology. To have captured means to have made captive. This can trammel the thinking, channelize research, and, by preventing new imagination, block progress.

Now psychology, as a science, is comparatively young. The science of the whole personality is the youngest child of this young science. It is characteristic of the young to look with reverence on, and worshipfully obey, their elders. So psychology and the study of personality obediently followed in the footsteps of their predecessors and attempted to build structures on the old pattern—nomothetic. Fortunately, children not only obey; they also rebel. However, in the present case I detect no hostility to the parental science figure, but a departure—a branching off. It is a sign of good health, in both parent and offspring, when the latter departs from familiar pathways, and the former can see him do it with equanimity.

SUB- AND SUPRAPERSONALITY RESEARCH

This datum, the whole personality, is, to a considerable group of investigators, including some in closely associated fields, the basic one in human behavior. It is the only *raison d'être* for the science of psychology. All other branches within this field are so many roads that lead to it. They do so from two directions.

One is at a "subpersonality" level; the other, "suprapersonality." In either event, they are significant sciences only in so far as they throw light on the behaving human. Among the sciences that lead to it from subpersonality levels are neurology, physiology, experimental psychology, psychometric tests. Specific researches that would be grouped under these heads would include those in conduction, the synapses, EEG, reflexes (unconditioned), all the receptor processes, psychophysics, the conditioned response, the functions and effects of such structures as the thalamus, adrenal cortex, autonomic nervous system, localization of function in the brain, various effects of nutrition, problems relative to sleep, etc. Some perception experiments belong here, although the trend now is to see perception as activity of the whole person.

Among those sciences that lead to the study of personality from the *supra* level are: anthropology, sociology in the older sense, social work as applied in community institutions, and the current newer disciplines known as group dynamics or action research. Here I would specify all studies in cultures as bearing on the personality or on formation of character; the developmental stages of the personality, including infancy, childhood and adolescence, maturity and old age, the special methods of studying the child—e.g., play techniques—would belong here. All the "functional" clinical pictures as, in fact, response to the environment would group here: the neuroses and the psychoses, and also the psychoanalytic formulations concerning them. Here

too belong the studies of special traits or behavior clusters, e.g., jealousy, suggestibility, masculinity, femininity, and sexual behavior. Also, here are such complex activities as delinquency, moral attitudes, aesthetics, religious systems. Finally, there is the individual in his industrial and business relations, in his political behavior, in the military services.

These lists do not pretend to be a systematic or exhaustive synopsis. They are more of the nature of random jottings. A complete statement would embrace all the efforts of psychologic science. Many psychologists will disagree. They will question whether some of these efforts properly belong within the curricula of a university department of psychology. Some are not quantifiable, or very slightly so; hence, are they science? The other horn of the psychologist's dilemma is: he collects data—rigorously controlled—enabling him to predict, but only within the narrow confines of the conditions set up in the experiment. How applicable are these to the essential problems of human life? It all comes down to the question as to what is the field proper of the science of psychology. It is a question on which there are both ponderings and searchings in important centers for the study of psychology.

To the student of the personality as a whole, the deficit in the nomothetic technique is that it dictates turning one's back on this unit datum. Variables are observed one by one. Each individual is measured in respect to only one such trait at a time, never in the interaction of the traits within him. The individual is atomized; and we stop with studying the atom. This is a product of the intellectualist temper, which has been under severe critical attack, especially from within philosophy (Lotze, James, Bergson). Psychology has been a closely guarded prisoner of the intellectualist temper.

Regarding the individual variable, some indirect light on its insufficiency comes from a recent paper on semantics. Rapoport (4), in the *American Scientist*, speaks of the "propositional function" of a statement. For example, the statement "the man is brave" looks like a logical proposition. But it cannot in itself be considered true or false unless the meaning of the predicate "brave," i.e., its function, is known. The man may be brave in battle . . .

> ". . . Jealous in honour, sudden and quick in quarrel,
> Seeking the bubble reputation
> Even in the cannon's mouth. . . ."
>
> . . . yet a coward in love (Miles Standish), . . .
>
> "I'm not afraid of bullets, nor shot from the mouth of a cannon,
> But a thundering 'NO,' point-blank, from the mouth of a woman,
> That, I confess, I'm afraid of."

He may eschew primitive hand-to-hand combat and be able to face the more deadly enmity of society (Mahatma Ghandi and many of the martyrs of religion).

Similar reasoning holds for any test that attempts to explore a whole personality, any multivariate instrument such as the Rorschach test. Any statement concerning any Rorschach test variable needs to be examined for its "propositional function." What we find out about a person in any one score is not necessarily true, and not necessarily false. We need to know its function in so far as that depends on its setting. We must therefore know that setting, the larger whole, if his behavior is to be predictable. At the present, the techniques used are Rorschach's test, Murray's TAT, other projective tools, the MMPI. Possibly some new kind of test, yet to be devised, will be more suc-

cessful. What ultimately happens to any now in use is not the important thing. It is important to have instruments that will dependably penetrate the whole person, as a psychodynamic system of stresses. Psychology can be depended on to devise them. It did so in psychometric science. The growth and now weighty influence of the Freudian hypotheses have given rise to the projective tests. But while the psychometric techniques did not present any challenging statistical difficulties, the projective instruments forthwith generated new problems—those of testing the test. You are familiar with the controversies. The next phase, in which we are now, was to throw the issue into the laps of the logicians of science.

THE USES OF IDIOGRAPHY

The new approaches are designated by the term "idiographic." The essence of these is that they permit the science of personality to keep in its syllogisms both terms, *science* and *personality*. A criticism—even stricture—of the nomothetic is that it can stick to science only at the expense of personality. In fact, the more scientific it is, the more isolated the variable it studies, and the less it takes in of the personality. But it is the human personality which is the objective of psychology. All the other sciences, to repeat, are *sub* or *supra*.

The essence of the idiographic method is that it focuses its glass on the universe of behavior traits co-functioning as a universe. This is the individual. But before it can be employed, a new question is legitimately posed and thrown back at the idiographist: how can we know—in the Rorschach test, for example—whether a productivity of, say, 30 or 50 is high or low? The same for an $F+$ per cent of so-or-so much, and the others. You cannot make the evaluations until we have criteria obtained by measuring each variable as distributed within a population. This is to fall back on the nomothetic approach. That is a first step. It must precede the idiographic.

The nomothetic is essential in setting up certain limits of quantitative findings. Doing this in psychology provides only certain directions of measurement, not absolute foot-rule figures. The directions of these measures in some populations—say normals or neurotics, or in children of one rather than another age level—and the differential directions in the other populations result in leads as to the organization of the variables in each clinical population. This organization or clustering is the raw material for a hypothesis to the effect that any one person within a clinical population would yield, in the variables sampled by the particular test, measures approximating certain limits. Conversely, a person found to measure in a number of these variables at or near certain quantitative limits belongs in the corresponding clinical population. At this stage, and for the purpose of experiment, the description of any person in the language of the psychological instrument used is itself a hypothesis. The next job becomes that of testing the hypothesis. This, much simplified, is the idiographic method. To test the hypothesis, we use all the usual methods of statistics. One of these, factor analysis, so employed becomes more directional than are its present habits. It circumscribes its individual tasks, does not take broadside shots, but "draws a bead," if you will, on certain objectives already found to be of potential significance.

To recapitulate, the steps in a scientific investigation of personality—after the test instrument has been chosen—are three. (*a*) The various behavior components accessible to the instrument are measured, and their distributions

plotted in the several clinical populations. This is nomothetic. From this step certain hunches emerge—tentatively we call them assumptions—as to the psychologic significance of the several components. (*b*) Individuals are studied by means of the test. Certain inferences as to dynamic relations between psychologic components within the individual follow—conclusions as to the mutual influences of the variables on one another and in producing the total picture. For example, lively imagination with too little appreciation of reality emerges as schizophrenia. The hypotheses that form immediately point the way to experiment, for the effect of the independent phenomenon on the dependent one: of schizophrenia, for example, on the pattern in a test such as the Rorschach. The third step is, therefore, (*c*) the experimental investigation itself. It obtains response patterns—the dependent structures—from individuals illustrating various independent conditions (neuroses, normal behavior, and the others) and tests these by the tried methods of investigation, correlation, factor analysis, analysis of variance, and the others. It is at point (*b*) that the method becomes idiographic.

THE LIMITATIONS ON NOMOTHESIS

The deficit of the nomothetic approach is that the more thorough and exact it is in adhering to its principles, the less adequate it is for exploring the whole functioning human unit. For, as it uses an ever larger and larger number of observations of one variable in different individuals, it reduces to a minimum the effects of other traits in the individual on that particular variable. So it obtains a datum that can be described independently of its shaping or distortion by each whole person. The greater the number of individuals in whom the trait was observed, the smaller is the standard deviation. So, the greater the "objectivity." We have eliminated the extravariable influences on the variable—extravariable, but intrapersonal—which is to say, we have eliminated the person. That is exactly what we want to do in such an experiment. The datum that is external to the rest of the personality is the datum of scientific interest of the moment. We take the person out of the observed datum, and concentrate only on the extraperson event. This is what scientific psychology is succeeding in doing. It is taking the human being out of the investigation. Any psychologist has the right to do so. Any investigator has the liberty of staking out his ground, defining his area, and limiting his objective. He must only be explicit in stating his definitions and his objective. What questions is he undertaking to answer?

The corollary that the unity of the person is a function of the brain acting as an organ, and an integrating one, is implicit in the thinking of some of the profound investigators of this organ. I mention here Sherrington, Herrick, and Lashley. A very recent paper by Sperry is apropos. His subject is "The Mind-Brain Problem." He concludes: "We should not expect to find that a single neuron or an isolated patch of neurons, or even a cortical center, could sense, feel, experience, or think anything in isolation. These psychic properties we envisage as depending upon a specific design and complexity in the vortex of neural activity, generally involving a reciprocal interplay of many parts" (5, pp. 310–311).

A universe of traits, variables in mutual interplay, affecting one another, these are the individual. This is the task which the idiographic method undertakes. The specific technique de-

vised to test out the findings in this kind of universe is that associated with Stephenson—the *Q* technique. It is to him, too, that I am indebted for the concept of the individual as a universe of traits. To quote his terse statement distinguishing the two methods, "Nomothesis was long regarded—and in effect, is—'psychology without a subject,' whereas idiography tried to concern itself with a self" (6).

The *Q* technique was the one I used in two researches in schizophrenia, one in adults and one in children (1947–1952). We have succeeded in these researches in isolating six schizophrenic reaction patterns. That is, we are describing six patterns within this disease group that differ from one another. They differ in the language of the Rorschach test items. They differ also in the corresponding clinical language. They make clinical sense. Full report of these researches will be published separately. The psychologist will be disappointed, however, who looks for descriptions of personality in terms of pure mathematics, logical deductions from quantitative findings, and from inferred relationships. Our findings do not provide such. Nor do I think that the science of personality by any technique has made progress such that it can so describe the personality. The science is in just too undeveloped and unsophisticated a state. I here quote another writer, Webster, discussing not the relatively complex field of personality as a whole, but psychology generally. He says: "Mathematical theory which is appropriate for interrelating many quantitative, continuous variables, is readily available, but there are always difficulties in applying it with any rigor to psychological data. This seems to be due mainly to the problems of measurement and possibly also to the fact that psychological variables may contain discontinuities which are poorly understood, and which may therefore vitiate the ordinary methods of analysis" (7, p. 168). What would be the ideal of such a science is stated by Boring in an article not *ad hoc*, but containing relevant comments: "The body of exact quantitative knowledge that we call science nowadays . . . the scientific psychology of the visual world, differs from phenomenology in being a collection of observed functional relations. . . . You cannot see the visual world at any moment when you are playing scientist; you construct it out of elaborate observations that have been collected for many years in the past" (1, p. 146).

The "discontinuities" of which Webster speaks are only too well known among the variables in which the clinical psychologist must deal. Hallucinations offer a case in point. Campbell, in one of his shorter papers (2), describing a girl of nineteen who appeared to be hallucinating, notes the difficulty of differentiating between her experiences as actual perceptions, or work of the imagination. "The patient does not make a consistent integration of different attitudes, but allows them to exist side by side," he says. Again: "Such a condition is not fixed and static; there is an oscillation between the different levels of thought. . . ." Such being the phenomenon which the clinical psychologist attempts to study, he does not have the aspiration as yet to describe them mathematically so as to answer Boring's criterion, "a body of exact, quantitative knowledge that we call science." In our idiographic study of schizophrenia, in patterning out the six types, we do not even claim that there is exact correlation between Rorschach test operations and the presumably corresponding clinical behavior. What we do say—and this is the meaning of our idiographic experiment—is that we can describe by means of the test the kind of whole person who, under certain

stress, or in one of Lewin's "field" conditions, is likely to have hallucinations.

Mention of Kurt Lewin's name recalls a quotation from him. Students in the field of personality, conscious as they are of the crudity of their instruments, of the long lag yet ahead before they can logically deduce constructs from mathematical findings, will find comfort in these words, coming from a man whose life was dedicated to the one goal, a science of personality. Lewin sees ". . . the basic character of science as the eternal attempt to go beyond what is regarded scientifically accessible at any specific time. To proceed beyond the limitations of a given level of knowledge, the researcher, as a rule, has to break down methodological taboos which condemn as 'unscientific' or 'illogical' the very methods or concepts which later on prove to be basic for the next major progress" (3, p. xv).

We may therefore expect more and more recourse to the idiographic approach, not only in psychology, but also in the other sciences concerned with human behavior. Idiography cannot do it alone. It must have a preceding groundwork by nomothesis. It is from the integrated use of the two methods that we will make progress. Out of such a synthesis should come a science of personality that will effectively retain both terms of this proposition: personality and science.

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LEARNING AND THE PRINCIPLE OF INVERSE PROBABILITY [1]

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The basic idea which has guided the theoretical explorations contained in this paper is that *science is a way of learning*. This thesis and some of its implications have been discussed in an earlier paper (1). In this paper the attempt is made to gain an understanding of the nature of the learning process through the examination of one formulation of the nature of the scientific method, the principle of inverse probability. This principle is best stated in its mathematical form, which will be found below. In brief, it formulates the effect on the probability of a theory of a confirmation. The principle formulates one important aspect of the scientific method. As such, it should tell us something concerning the psychology of learning.

In a sense, what follows can be considered to be the elaboration of Hull's "hidden" theory of learning; for Hull has not one but two theories of learning. One of these is that which is generally understood to be Hull's theory; the other is contained in his discussions on method, particularly the introductory sections of (8) and (9), and can be called the "hypothetico-deductive theory of learning." The latter is a theory of how the scientist learns. In this latter theory the three major concepts are *theory*, *observation*, and *probability*. Probability is what characterizes the relation between theory and observation. Thus, on the grounds of certain observations, a theory is said to have such and such a probability. On the grounds of a given theory, a predicted observation has such and such a probability of occurring. When a predicted observation is verified, the probability of the theory goes up. When a predicted observation fails to be verified, the probability of the theory goes down.

This "hidden" theory of learning is more of a cognitive than an S-R theory. It points almost directly to the principle of inverse probability, with which we shall deal below. It is to this principle that we turn for a fuller understanding of the hypothetico-deductive theory of learning and for its formalization.

The Principle of Inverse Probability in General

According to Jeffries (11), the principle of inverse probability "is to the theory of probability what Pythagoras's theorem is to geometry." Although the principle is one of the most critical bones of contention among probability theorists, it is mathematically sound. This is attested to by Uspensky, who says "Bayes' formula, and other conclusions derived from it, are necessary consequences of fundamental concepts and theorems of the theory of probability. Once we admit these fundamentals, we must admit Bayes' formula and all that follows from it" (17, p. 69). The major question is its applicability.

Probability theorists fall into two general categories, frequency theorists

[1] The writer is deeply indebted to the Research Council of the University of Missouri for a research professorship for the summer of 1952 which made the investigations reported in this paper possible.

and nonfrequency theorists.[2] The frequency theorist insists on an "objective" definition of probability in terms of relative frequency. The nonfrequency theorist considers probability to be a kind of rating scale of credence. Probability is, according to the nonfrequency theorist, "The state of mind with respect to an assertion, a coming event, or any other matter on which absolute knowledge does not exist" (**4**). The basic syntactical unit for the nonfrequency theorist is $P(g/h)$,[3] the probability of g on the grounds of h. If g necessarily follows from h, as in deduction, then $P(g/h) = 1$. If g is impossible on the data h, then $P(g/h) = 0$.

It is important to specify the point of view of this paper with respect to this controversy. On the philosophical level no position is taken. The acceptance of the principle is only on the grounds of its cogency with respect to what it might mean for the psychology of learning.

Mathematical Formulation of the Principle of Inverse Probability

The principle of inverse probability formulates the effect on the probability of a proposition g of the verification of a proposition x. Let us say that, on the basis of a previously acquired set of propositions h, a proposition g has the probability $P(g/h)$. Then, say, a proposition x is found to be true. What is the effect of finding x to be true on the probability of g? A root form[4] of the principle (**11**) provides us with the answer:

$$P(g/hx) = \frac{P(x/gh)}{P(x/h)} P(g/h). \quad [1]$$

The probability that g is true on the grounds of h and x is equal to the probability that g is true on the grounds of h, multiplied by the ratio of the probability of x on g and h, to the probability of x on h (h alone).

Thus, if x stems equally well from g and h, as from h alone, the probability of g does not change. If, however, x follows from g and h with a probability greater than the probability of x on h alone, the probability of g increases. Similarly, if $P(x/gh)$ should be less than $P(x/h)$, the probability of $P(g/hx)$ is less than $P(g/h)$.

The nature of this formula will become evident if we make the following interpretations of g, h, and x: Let h be a set of propositions concerning data which have been collected; let g be a theory which has been developed to account for the data h; and let x be a proposition about a new datum which was not involved in the initial generation of g. The formula then expresses the most critical aspect of the hypothetico-deductive method,

[2] Nagel (**15**) has recently summarized the controversy. Nagel himself accepts the frequency point of view and presents a rather weak case for the nonfrequency point of view. A balanced picture of the situation can be had by reading Nagel on the controversy in general, supplemented with, perhaps, Keynes's (**13**) presentation of the nonfrequency point of view.

[3] The symbol "/" should not be confused with this symbol as used to indicate division. It does not indicate division as used here. It should be read as "on" or "on the grounds of."

[4] This is immediately derivable from the theorem of compound probability. This theorem is (**17**):

$$P(ABC) = P(A)\,P(B/A)\,P(C/AB). \quad \text{(a)}$$

We then have

$$P(hgx) = P(h)\,P(g/h)\,P(x/gh) \quad \text{(b)}$$

and

$$P(hgx) = P(hxg) = P(h)\,P(x/h)\,P(g/hx) \quad \text{(c)}$$

and

$$P(g/hx)\,P(x/h) = P(x/gh)\,P(g/h) \quad \text{(d)}$$

and therefore

$$P(g/hx) = \frac{P(x/gh)}{P(x/h)} P(g/h). \quad \text{(e)}$$

the effect of the confirmation of a deduction on the probability of the hypothesis that generated it. If x is found to be true, and x stems from the theory with a relatively high probability, then the probability of the theory is raised.

By appropriate algebraic manipulation,[5] formula [1] leads to

$$P_n = \frac{R\,P_{n-1}}{R\,P_{n-1} + (1 - P_{n-1})}, \quad [2]$$

where P_n is the probability of g after the verification of x_n, and P_{n-1} is the probability of g prior to the verification of x_n, and R is the ratio of $P(x_n/gh)$ to $P(x_n/\bar{g}h)$. ($\bar{g}$ = not $-$ g.)

THE INTERPRETATION OF g, h, AND x

An interpretation of g, h, and x as propositions concerning theory, old data, and new datum, respectively, has already been given. However, for the principle of inverse probability to have greater generality for the psychology of learning, we will widen the interpretation of these symbols. The crux of this wider interpretation is in the interpretation of x, with which we shall deal first.

Let x stand for the conditional proposition, "If Y, then X," where X is an observation on the part of the learning organism, and Y the conditions for the observation. By the verification of x, we mean the verification of this whole conditional proposition. Some examples may be helpful:

Instrumental conditioning: x = "If I press the bar, I will get a pellet of food."

Classical conditioning: x = "If the metronome ticks, meat powder will be injected into my mouth."

Problem solving: x = "If I do such and such, the obstacle will be overcome."

Prejudice: x = "If he is a Negro, then I will find him to be irresponsible."

Law: x = "If this body is immersed in this liquid, then it will be buoyed up by a force which is equal to the weight of the liquid it displaces."

With this as our understanding of the meaning of x, *g is, then, whatever it is that generates x's.* g is that which is associated with the organism when we say that the organism has learned. It is the "what is learned" in the sense that we say that habits, attitudes, prejudices, skills, cognitions, hypotheses, etc. are learned. It should be emphasized that g is in no sense the overt behavior, but is, rather, the condition of the organism. The essential characteristic of g is that it can generate x's.

The h is whatever led to the g. It consists of hereditary factors, maturational factors, and previous learnings.

THE AMOUNT OF LEARNING AS A FUNCTION OF ONE POSITIVE "REINFORCEMENT"

"Reinforcement" may now be defined as the verification of x. We are now in a position to determine the amount of increase in the probability of g as a function of one reinforcement.

The amount of increase is given by the increment in the probability of g.

$$P_n - P_{n-1} = \frac{R\,P_{n-1}}{R\,P_{n-1} + (1 - P_{n-1})} - P_{n-1}. \quad [3]$$

[5] From formula [1] we have

$$P(g/hx) = \frac{P(x/gh)}{P(x/h)}\,P(g/h) \qquad (a)$$

and therefore also

$$P(\bar{g}/hx) = \frac{P(x/\bar{g}h)}{P(x/h)}\,P(\bar{g}/h). \qquad (b)$$

By dividing (a) by (b), with

$$P(g/hx) + P(\bar{g}/hx) = 1,$$

with $P(g/h) + P(\bar{g}/h) = 1$, and letting

$$\frac{P(x/gh)}{P(x/\bar{g}h)} = R,$$

we get

$$P(g/hx) = \frac{R\,P(g/h)}{R\,P(g/h) + [1 - P(g/h)]}. \qquad (c)$$

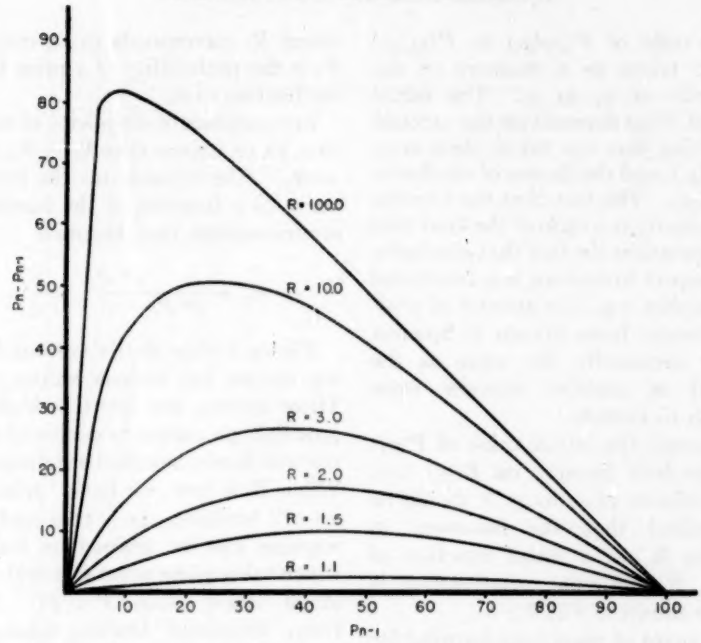

FIG. 1. The amount of increment, $P_n - P_{n-1}$, as a function of one "reinforcement," as a function of P_{n-1} and R

In Fig. 1, the values of the increment have been plotted for various values of R as a function of P_{n-1}. For any value of R which is greater than 1, the increment is low for both low and high values of P_{n-1}, and high for middle values of P_{n-1}.

Immediate empirical confirmation of the relationship indicated by this figure is to be found in phenomena of transfer of training. Roughly, where an individual approaches a "new" learning situation without already having very much to transfer to it, the initial learning of the new situation is slow, i.e., when P_{n-1} is low, $P_n - P_{n-1}$ is low. If, however, P_{n-1} is a middle value, improvement is rapid; and if P_{n-1} is already high, there is little more room for improvement. The work of Harlow (5) with respect to the learning of learning sets is a case in point. Translating his results into the terminology advanced here, where the initial probability of g is low, the increments as a function of reinforcement are low. Where the initial probabilities are of middle values, the increments are highest. Where the initial probabilities are high, the increments go down again. Or, what is the same thing, when P_{n-1} is low, learning is positively accelerated; and when P_{n-1} is high, learning is negatively accelerated.

The phenomenon of positive transfer of training can be further specified. From the mathematics of probability we have the formula: [6]

$$P(g_2) = P(g_1)\frac{P(g_2/g_1)}{P(g_1/g_2)}. \qquad [4]$$

[6]
$$P(g_1 g_2) = P(g_1)\,P(g_2/g_1) \qquad \text{(a)}$$
$$P(g_1 g_2) = P(g_2 g_1) = P(g_2)\,P(g_1/g_2) \qquad \text{(b)}$$
$$P(g_2)\,P(g_1/g_2) = P(g_1)\,P(g_2/g_1) \qquad \text{(c)}$$
$$P(g_2) = P(g_1)\frac{P(g_2/g_1)}{P(g_1/g_2)}. \qquad \text{(d)}$$

The ratio of $P(g_2/g_1)$ to $P(g_1/g_2)$ can be taken as a measure of the similarity of g_2 to g_1. The initial value of $P(g_2)$ depends on the amount of learning that has taken place in g_1, i.e., $P(g_1)$, and the degree of similarity of g_2 to g_1. The fact that the formula for similarity is a ratio of the kind that it is dramatizes the fact that similarity with respect to learning is a directional relationship, e.g., the amount of positive transfer from French to Spanish is not necessarily the same as the amount of positive transfer from Spanish to French.

Although the initial value of $P(g_2)$ is dependent linearly on $P(g_1)$ and the similarity of g_2 to g_1, it should be emphasized that the *increment* in learning is not a linear function of $P(g_1)$. Rather, the relationship is that as shown in Fig. 1.

The point of maximum learning for one reinforcement as a function of P_{n-1} varies with R. When R is high, the maximum is at low values of P_{n-1}. When R is low, the maximum is at higher values of P_{n-1}, although it is always at a value of P_{n-1} which is less than .50. Specifically, $P_n - P_{n-1}$ is maximal when [7]

$$P_{n-1} = \frac{\sqrt{R} - 1}{R - 1}. \qquad [5]$$

CURVES OF LEARNING

For the purposes of showing the progress in learning, formula [2] can be generalized to the form

$$P_n = \frac{R_1 R_2 \cdots R_n P_0}{R_1 R_2 \cdots R_n P_0 + (1 - P_0)}, \qquad [6]$$

where R_1 corresponds to x_1, etc., and P_0 is the probability of g prior to the verification of x_1.

For purposes of simplicity of exposition, let us assume that $R_1 = R_2 = \ldots = R_n$. The formula for the learning curve as a function of the number of reinforcements then becomes

$$P_n = \frac{R^n P_0}{R^n P_0 + (1 - P_0)}. \qquad [7]$$

Figure 2 then shows various learning curves for various values of R. These curves run from S-shaped to growth-type curves to curves of sharp rise which may be called insight curves. When R is low, we have "trial-and-error" learning, i.e., trial-and-error learning can be defined as learning which takes place when $P(x/gh)$ is not much larger than $P(x/\bar{g}h)$. Similarly, "insightful" learning takes place when $P(x/gh)$ is considerably larger than $P(x/\bar{g}h)$.[8]

EXTINCTION

By analogous reasoning, it is possible to derive the phenomenon of extinction. Extinction is the consequence of the verification of $\bar{x}$. The result on the probability of g of the verification of $\bar{x}_m$ is

$$P_m = \frac{P_{m-1}}{P_{m-1} + \bar{R}(1 - P_{m-1})}, \qquad [8]$$

where m is the number of extinction trials, P_m is the probability of g after

[7] This was obtained by differentiating $P_n - P_{n-1}$ with respect to P_{n-1}, setting the derivative equal to zero, and solving for P_{n-1}. It has a limit of .50 as R approaches 1, R being 1 or greater. When $R = 1$, the maximum is indeterminate.

[8] Cf. Harlow: "The very form of the learning curve changes as learning sets become more efficient. The form of the learning curve for the first eight discrimination problems appears S-shaped: it could be described as a curve of 'trial-and-error' learning. The curve for the last 56 problems approaches linearity after trial 2. Curves of similar form have been described as indicators of 'insightful' learning" (5, p. 53).

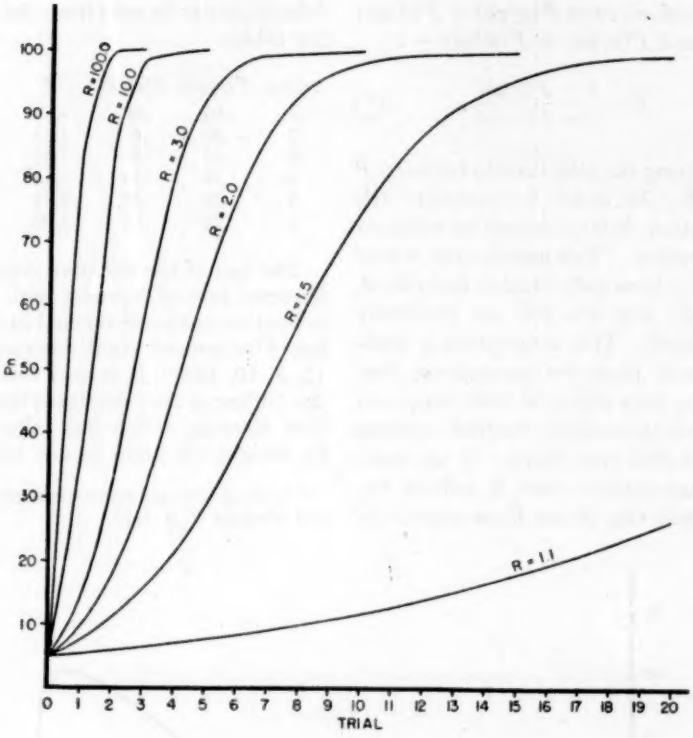

FIG. 2. Learning under a constant R

the verification of $\bar{x}_m$, P_{m-1} is the probability of g prior to the verification of $\bar{x}_m$, and $\bar{R}$ is the ratio of $P(\bar{x}/gh)$ to $P(\bar{x}/gh)$. Figure 3 shows the effect of one negative reinforcement, the verification of $\bar{x}$ (x being found false). The equation for P_m, the probability of g after m successive extinction trials, is

$$P_m = \frac{P_0}{P_0 + \bar{R}_1 \bar{R}_2 \cdots \bar{R}_m (1 - P_0)}. \quad [9]$$

Figure 4 shows the extinction curves for constant values of $\bar{R}$.

DEDUCTIONS

It is beyond the scope of the present paper to elaborate on all the deductions that may stem from the theory of learning derived from the principle of inverse probability. However, in order to demonstrate its cogency, one example will be given.

What is the relation between the rate of learning and the rate of extinction? We have seen that if R is high the rate of learning is high, and that if $\bar{R}$ is high the rate of extinction is high. The question becomes one of determining the relationship between R and $\bar{R}$.

$$R = \frac{P(x/gh)}{P(x/\bar{g}h)}, \quad [10]$$

$$\bar{R} = \frac{P(\bar{x}/\bar{g}h)}{P(\bar{x}/gh)}. \quad [11]$$

Therefore, since $P(x/gh) + P(\bar{x}/gh) = 1$, and $P(x/\bar{g}h) + P(\bar{x}/\bar{g}h) = 1$,

$$\bar{R} = \frac{1 - P(x/gh)}{1 - P(x/\bar{g}h)}, \qquad [12]$$

thus fixing the relationship between R and $\bar{R}$. In order to complete the derivation, it is necessary to make an assumption. This assumption is that as we go from individual to individual, $P(x/gh)$ and $P(x/\bar{g}h)$ are positively correlated. This assumption is nothing more than the assumption that persons vary either in their supply of g's, or in their ability to draw x's from the g's that they have. If we make this assumption, then it follows immediately that R and $\bar{R}$ are negatively related, as can be seen from the following table:

Subject	$P(x/gh)$	$P(x/\bar{g}h)$	R	$\bar{R}$
1	.95	.80	1.19	4.00
2	.80	.65	1.23	1.75
3	.65	.50	1.30	1.43
4	.50	.35	1.43	1.30
5	.35	.20	1.75	1.23
6	.20	.05	4.00	1.19

The fact of the negative correlation between rate of learning and rate of extinction has been verified in a number of instances in conditioning studies (**2, 3, 10, 14**).[9] It is also verified by the finding of the Gestalt psychologists that learning which has taken place by insight (R high) is not forgotten

[9] These studies are summarized by Hilgard and Marquis (**7**, p. 119).

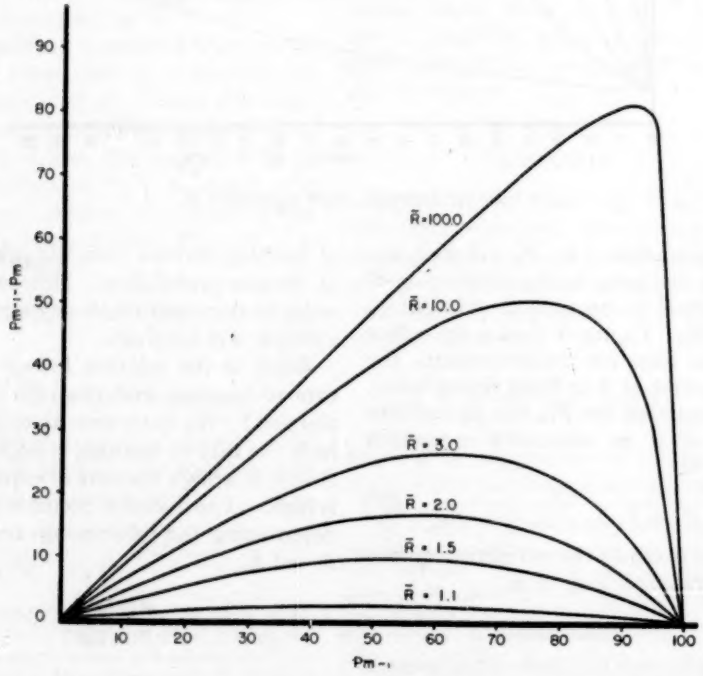

FIG. 3. The amount of decrement, $P_{m-1} - P_m$, as a function of one extinction trial, as a function of P_{m-1} and $\bar{R}$

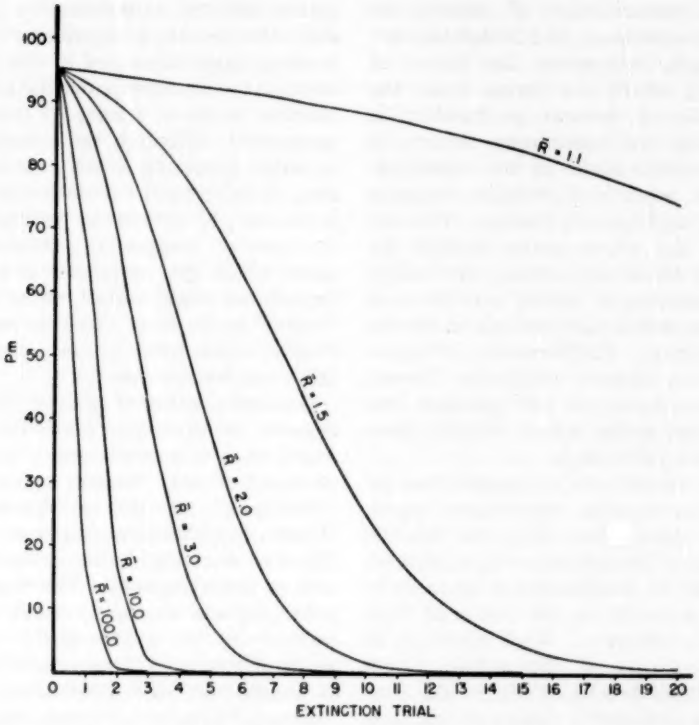

FIG. 4. Extinction under a constant $\bar{R}$

as quickly as learning which has taken place by rote (R low) (**12**), if we can, at least for the present, equate forgetting with extinction.

THEORETICAL IMPLICATIONS

As has been indicated, the primary purpose of this paper is simply to show the possibility of developing a theory of learning on the basis of the principle of inverse probability. However, taking the principle of inverse probability as a theory of learning has a number of implications with respect to psychological theory.

The method of science as a way of learning. In the opinion of the writer, the most important implication inherent in what has already been said is that by viewing science as a method of learning it is possible to learn something concerning the nature of learning. Whether the principle of inverse probability fully stands up against further scrutiny and empirical data with the interpretations which we have given to g, h, and x is to be seen. The burden of this paper is primarily to show the possibilities which are inherent in viewing science as a way of learning. Parenthetically it should be pointed out that the very testing of what has been advanced in this paper will employ the principle of inverse probability although it may not employ the particular interpretations of g, h, and x which we have advanced.

The reconciliation of stimulus-response, expectancy, and Gestalt theories. Although, in a sense, this theory of learning which we derive from the principle of inverse probability is primarily an expectancy theory, it incorporates some of the most important aspects of stimulus-response theory and Gestalt theory. For one thing, the phenomenon critical for Gestalt theory of learning, the insight phenomenon, is readily yielded as a consequence of the principle of inverse probability. Furthermore, it specifies, in a manner which the Gestalt theorists have not yet specified, the condition under which insight takes place, i.e., R is high.

One of the major superiorities of stimulus-response theory over expectancy theory has been the relative docility of the stimulus-response point of view to mathematical quantification, primarily in the hands of Hull and his followers. With the kinds of interpretation of the values which have been developed within the context of probability theory as has been employed here, the mathematics of probability becomes readily available for the handling of psychological problems. From a mathematical point of view, there is also a distinct superiority in the kind of quantification which has been outlined here. The heart of Hull's theory is in the equation for the learning curve. This was arrived at primarily on the basis of the relatively arbitrary method of curve fitting. There is really no good a priori rationale for determining whether the curve of learning should be a logarithmic or exponential growth function, as indicated by Hull's own wavering in this matter. On the other hand, the curve of learning which has been advanced in this paper is *necessary* on the basis of the fundamental postulates of probability theory and the interpretation given. One other point of superiority of the learning curve advanced in this paper over that of Hull's exponential growth function is, so to speak, its intuitive propriety. Although the criterion of intuitive propriety is not a necessary one, it is certainly a desirable one. Equation [1] (or better, perhaps, the theorem of compound probability), upon which the remainder is based, formulates what we all, in a sense, "know" to be true. In this respect, Hull's exponential growth function falls considerably short.

Account is taken of effect of "belongingness" on learning. Thorndike (**16**) found that it was necessary to take account of the "belongingness" or "relevance" of the reinforcement. When there was belongingness, learning was more rapid than when there was no belongingness. The degree of belongingness is exactly what is expressed by the value of R. If the value of $P(x/gh)$ is large, and $P(x/\bar{g}h)$ is small, then the verification of x "belongs" to g, or is relevant to g, and consequently, as has been indicated, the rate of learning is high.

Consistency with the general nativism of other theories. According to Thorndike and others, what is learned has, so to speak, to exist, at least in small measure in the organism. Thus, in Thorndikean terms learning consists in the strengthening of *already existing connections*. If the connections do not exist, at least in small measure, then they cannot be reinforced, and learning cannot take place.

In this respect, the present theory has to assume the very same thing. If learning is the growth of the probability of g, then there must be an initial probability of g which is greater than zero. If the initial probability of g is zero, then there can be no increase in the probability of g [3].

The possibility of understanding "complex" learning phenomena. Hilgard (6), in his opening paragraph, suggests that a theory of learning ought to be able to account for such phenomena as "prejudice and bigotry and other learnings which lead to trouble instead of to a satisfactory solution of . . . problems," as well as how skills, preferences, tastes, and knowledge develop. In most learning theories, these "complex" learnings are looked upon as a kind of higher level problem which will be solved after we have solved the problems involved in less "complex" learning. In the theory outlined, it is not necessary to differentiate between lower and higher learnings. The theory applies equally well to the learning of a conditioned response or a prejudice. The issue of molarity versus molecularity vanishes, as does the question as to whether reasoning and problem solving ought or ought not to be considered categories under the general heading of learning. Nor is it necessary to introduce the distinction between primary and secondary reinforcement to explain other than very simple learning.

HILGARD'S SIX QUESTIONS

Hilgard (6) has formulated six questions about theories of learning which are appropriately asked whenever a "new," or new version of an "old," theory is advanced. The attempt will be made to answer these questions with respect to what has been advanced here.

1. "*What are the limits of learning?*" Although the theory does not provide a contentual answer to this question, it supplies a formal answer. Learning is a function of $P(g/h)$, $P(x/gh)$, and $P(x/\bar{g}h)$. The limits of learning are therefore in terms of the limits of h, the generation of g's on the basis of h, the generation of x's on the basis of the g's and h's, and whatever is involved in the determination of whether an x is or is not to be "tried." (The latter word is borrowed from Hilgard (6) as he uses it in the concept of "provisional try.") Unless there are x's, and unless the occasion for the verification of x's occurs, learning will not take place.

2. "*What is the role of practice in learning?*" Practice consists in the confirmation and nonconfirmation of x's. Repetition which does not involve the testing of x's will not result in learning. The amount of practice and its quality, the latter defined as the attributes of x, and the associated value of R are determiners of the amount of learning.

3. "*How important are reward, punishment, or other motives in learning?*" They are important in so far as they provide a basis for verifying x's, whose X's involve reward, punishment, or other consequences.

4. "*What is the place of understanding and insight?*" Whether learning is "blind" or "insightful" is a matter of degree as reflected in the magnitude of R.

5. "*Does learning one thing help you learn something else?*" Yes, in so far as the former thing learned has affected the initial probability of g as indicated by formula [4].

6. "*What happens when we remember and when we forget?*" Thus far in our development of the theory, we have only one suggestion to make with respect to this question. It has been shown that on the basis of the theory it follows that there is an inverse relation between R, the rate of learning, and $\bar{R}$, the rate of extinction. Katona's (12) investigations have shown that if learning is based on principle (R high), there is less forgetting ($\bar{R}$ low). Similar confirmations of the inverse

relationship between R and $\bar{R}$ have been found in conditioning studies. Thus, according to this, we will remember best that which has been learned with R high.

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SIMULTANEOUS AND SUCCESSIVE DISCRIMINATION

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In a recent experiment by Weise and Bitterman (12) the relative difficulty of simultaneous and successive discrimination was studied. A four-unit apparatus of the alley-maze type was employed, with two small lamps mounted at each choice point. One group of rats was trained on the simultaneous problem (i.e., turn in one direction when the right-hand lamp is on and in the opposite direction when the left-hand lamp is on) and a second group was trained on the successive problem (i.e., turn in one direction when both lamps are on and in the opposite direction when both lamps are off). The first problem proved to be much more difficult than the second, a result of considerable theoretical interest.

As Weise and Bitterman noted, the influential theory of Spence (8) was unable to account for the fact that the successive problem could be learned at all. Furthermore, a logical extension of the theory in accordance with the Hullian principle of afferent neural interaction (4), while making an explanation of successive learning possible, would lead to the deduction that the successive problem should be more difficult than the simultaneous. The Weise-Bitterman paper led Spence to modify his theory in the manner anticipated (9).[1] At the same time he reported the results of a new experiment with a simple elevated T maze (gray stem and black or white arms) in which the simultaneous problem was found to be considerably easier than the successive. Spence emphasized the correspondence of the new results with his extended theory and dismissed the contradictory data in an offhand manner: "Just why Weise and Bitterman got opposite results is not clear, as it is difficult to interpret the very complex type of discrimination set-up they employed. The simple discrimination situation is sufficiently difficult to deal with theoretically without adding all of the problems that arise as the result of the serial nature of the multiple discrimination set-up along with the fact that it involves a gradient of reinforcement" (9, p. 91).

Weise and Bitterman noted also the bearing of their data on the validity of Nissen's (5) attempt to subsume all discriminative behavior under the general headings of approach and avoidance. Nissen had deduced that the successive problem should be more difficult than the simultaneous problem since the former requires the development of conditional reactions or stimulus-compounding, a deduction which seemed to follow logically from a literal approach-avoidance theory. In a subsequent commentary on the Weise-Bitterman experiment, however, Nissen (6) denied that its results had any bearing on the validity of his formulation and attributed his deduction to the "misplacement" of a sentence. From this paper it became clear that Nissen's conceptual scheme was so broad as to be incapable of experimental evaluation (1).[2] Nissen did,

[1] Successive discrimination was explained in terms of compounding and was assumed, therefore, to be more difficult than simultaneous discrimination. For an evaluation of the extended theory, see Bitterman (2).

[2] The test which Nissen himself performed (5) is deprived of conclusiveness by his analysis of the Weise-Bitterman experiment. Animals were taught a simultaneous white-black discrimination with the stimuli horizontally

nevertheless, make one important criticism of the Weise-Bitterman experiment. He suggested that the less rapid learning of the simultaneous problem might have been due to reduction of the bright-dark difference by reflected light. From this point of view, Spence's contradictory results might be attributed to his use of painted stimuli rather than to the simplicity of his method.

These considerations led us to perform further experiments on the relative difficulty of simultaneous and successive problems with the conventional jumping apparatus. That situation should be simple enough to meet the requirements of Spence's theory, and the use of painted stimulus cards should avoid the criticism of the Weise-Bitterman apparatus which Nissen proposed. An as yet unpublished doctoral dissertation of E. F. MacCaslin provided evidence in support of the prediction by Weise and Bitterman that the relative difficulty of the two problems would depend on the similarity of the stimuli to be discriminated; [3] however, while the successive problem proved to be much more difficult than the simultaneous problem when difficult discriminations were employed (e.g., two vertically striped cards differing in stripe-width), even with simple discriminations (e.g., horizontally vs. vertically striped cards) the simultaneous problem was somewhat easier than the successive. In a subsequent experiment by Bitterman, Calvin, and Elam (3) with a discrimination between two circles markedly different in diameter, successive and simultaneous groups performed in almost identical fashion,[4] but in no experiment did we find superior performance in the successive group.

These results led us to reconsider the earlier interpretation of the Weise-Bitterman data. Another look at the maze apparatus made Nisson's explanation in terms of reflected light seem unlikely, and Spence's comment on complexity did not seem to further our understanding of the contradictory results—why the use of a multiple-discrimination apparatus should affect the relative difficulty of the two types of problem was not clear. Upon further consideration, however, another interpretation of the divergent results occurred to us. Weise and Bitterman assumed that their successive problem had been mastered on a configurational basis—its relative simplicity led them to reject the idea of compounding or conditional discrimination in favor of the assumption that the animals had learned merely to make one response to the bright configuration and an opposed response to the dark—but they expressed some doubt as to how the simultaneous problem had been solved: "If learned configurationally, the greater difficulty of the simultaneous problems may be attributed to the greater similarity between the two stimulus-patterns

arranged and later tested with a vertical arrangement of the same stimuli. Had there even been zero transfer, Nissen could have dealt with the results by assuming that the animals had learned to "approach" and "avoid" brightness-position compounds which were disrupted by the shift in the locations of the stimuli.

[3] Nissen was unkind enough to suggest that this prediction was made in anticipation of "the possibility of experimental evidence inconsistent with" the position of Weise and Bitterman (12, p. 164). Actually, the prediction followed logically from the data of Saldanha and Bitterman (7) on relational learning.

[4] In this experiment the simultaneous group had previously learned a simultaneous problem (horizontal vs. vertical stripes) while the successive group had been trained on a corresponding successive problem. For naive animals trained by MacCaslin on the circle discrimination, the simultaneous problem was somewhat easier. These studies point to the operation of qualitatively distinct perceptual sets in the two types of problem.

which it presented to the animal. . . .[5] If mastery was based on the acquisition of functional properties by afferent components, this kind of learning may be assumed to involve a more complex, higher order process" (12, pp. 192–193). Although Weise and Bitterman were led to emphasize the second interpretation, the experiment of Spence and our later studies with the jumping apparatus required us to re-examine the first.

In the jumping apparatus the animals were required to jump directly at the stimulus cards, and in Spence's T maze the animals were required to enter upon the stimulus runways. These may be described as *approach* situations in the literal meaning of the word. The relative simplicity of the simultaneous problem in such situations can be explained on the assumption that they facilitate the functional isolation of the two members of each pair of stimuli. In the apparatus of Weise and Bitterman, on the other hand, the two stimuli (lamps) at each choice point were closely juxtaposed and the animals were required to turn away from them, to one side or the other, in making their way through the maze. This may be described as a *response* situation. It facilitates configurational organization and thereby retards the functional isolation of the two members of each pair of stimuli. From this point of view the greater difficulty of the simultaneous problem is understandable either in terms of the greater similarity of its two configurations (on the assumption that solution is based on configurational discrimination) or in terms of the difficulty of perceptual analysis in such situations (on the assumption that solution is based ultimately on response to components). This interpretation, like Weise and Bitterman's, proposes two qualitatively distinct types of perceptual organization in these problems, but the functional priority of configurational perception is not postulated. Instead, it is assumed that the dominance of one or the other kind of organization is determined by the physical characteristics of the apparatus employed.

The experiment to be reported was designed to test the hypothesis that the Weise-Bitterman results were due to the fact that their apparatus—presenting closely juxtaposed stimuli which the animals were not required to approach directly—favored configurational organization. To distinguish between this interpretation and those of Spence and Nissen, it was necessary only to reproduce the essential features of the Weise-Bitterman situation in a suitably modified jumping apparatus.[6]

METHOD

Thirty experimentally naive Albino rats bred in the laboratory were studied. They were between three and four months old at the start of the experiment. After adjustment to handling, the animals were placed on a 24-hr. feeding schedule and preliminary training in the apparatus was begun.

The jumping apparatus was designed in the conventional way with only one exception. The two windows to which the animals were to jump were separated by a center window which was used only for stimulus cards (Fig. 1). The three windows were cut in a hemihexagonal surround which was painted gray. Behind the windows was a feeding platform to which the animals gained access following a correct response, and below the windows

[5] It was this statement that Nissen (6) apparently confused with his own analysis in terms of reflected light. Weise and Bitterman were merely noting the obvious fact that the configurations *dark-light* and *light-dark* are more similar than the configurations *dark-dark and light-light*. That this difference is independent of the stray-light problem the present experiment (with painted stimuli) will demonstrate.

[6] We are indebted to Mr. Richard Gonzalez for assistance in the conduct of this study.

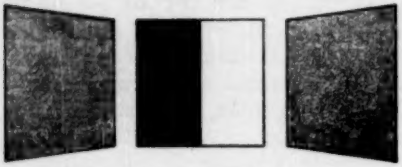

FIG. 1. Rat's view of the stimulus card (center) and response windows. The stimulus card shown was used in the simultaneous problem, in which the animal's task was to jump to one of the gray cards when the white area was on the right and to the other gray card when the white area was on the left. In the successive problem the stimulus card was either all white or all black, the white card signalling response to one of the windows and the black card to the other. The three windows in which the stimulus and response cards were set were cut in a hemihexagonal surround.

was a cloth net into which the animals fell after an incorrect response.

During preliminary training a gray card was locked in the center window, and the animals were trained to jump gradually increasing distances (up to a maximum of 9 in.) through the open left and right windows. Then they were trained to jump to unfastened gray cards (somewhat darker than the gray of the background to facilitate localization) in the left and right windows. Manual guidance was used to ensure equal experience with both windows. Following the preliminary training the animals were divided into two groups of 15 animals each which were matched for adjustment to the situation.

Group I was trained on the simultaneous problem and Group II on the successive problem. For both groups the gray cards used in the preliminary training appeared in the lateral windows on each trial. The center window, which contained the stimuli to be discriminated, was locked at all times as before. Group I was trained with a stimulus card which was half black and half white (Fig. 1). Seven of the animals in this group were rewarded for jumping to the right window when the white half of the card was on the right (black-white configuration) and to the left window when the white area was on the left (white-black configuration). The direction of correct response was reversed for the other eight animals of Group I (left to black-white and right to white-black). Group II was trained with two stimulus cards, both halves of each being either black or white. Seven of the animals were rewarded for jumping right to white-white and left to black-black, while the direction of correct response was reversed for the remaining eight animals of Group II (left to white-white and right to black-black). Ten trials per day, five to each of the two configurations of each problem, were given by the correction method. Each animal was allowed a maximum of three free jumps on each trial, and after three successive errors it was manually guided in the correct direction; a correct jump terminated each trial. The criterion of learning was one errorless day.

RESULTS AND DISCUSSION

The course of learning in the two groups is plotted in Fig. 2 (initial errors) and Fig. 3 (total errors). In Table 1 the results are summarized in terms of mean errors and days to criterion. The difference between the two groups was large and statistically significant for all measures. The direction of the difference was in accord with that found by Weise and Bitterman—the performance of the successive group being markedly superior to that of the simultaneous group—and there was as little overlap between the two groups in the present experiment as in the earlier

TABLE 1

RELATIVE DIFFICULTY OF SIMULTANEOUS AND SUCCESSIVE PROBLEMS

	I Simultaneous	II Successive	*Dif.**
Days	14.0	7.9	6.1
Initial errors	50.7	25.2	25.5
Total errors	80.9	40.1	40.8

* All differences were significant beyond the 1% level of confidence by Wilcoxon's nonparametric test for unpaired deviates (13).

one. Thirteen of our successive animals reached the criterion of learning before the first simultaneous animal had done so, while in the earlier experiment 9 of the 10 successive animals reached criterion before the first simultaneous animal.

Although the present experiment confirms the results of Weise and Bitterman, accumulated evidence requires a new interpretation of those results. As we have seen, Weise and Bitterman assumed that their simultaneous problem represented a simple within-pairs differentiation, and on this assumption the greater simplicity of the successive problem was taken as evidence for the relatively primitive nature (defined in terms of priority or dominance) of configurational perception. It now seems likely that *both* the Weise-Bitterman problems were configurationally organized, at least to begin with, and that the difficulty of the simultaneous problem was due to the greater similarity between the configurations of that problem or to the fact that those configurations could not be readily differentiated into components. In situations such as Spence's T maze or the conventional jumping apparatus, which require the animals to approach (jump at or enter upon) the stimuli more directly, internal differentiation of simultaneous configurations is more readily achieved, and the simultaneous problem seems to be less difficult than the successive. Configurational effects may, nevertheless, operate in such situations (3, 14), and under specialized conditions, such as "two-situational" problems in which between-pairs differences are great and within-pairs differences are small, configurational organization may predominate (10, 11). At the present time the emphasis of Weise and Bitterman on qualitatively distinct levels of perceptual organization seems to be justified; unfortunately, however, the precise nature of these processes, their interrelations, and their hierarchical arrangement on a scale of priority remain in considerable doubt.

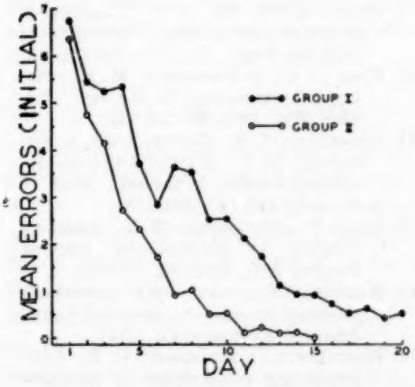

FIG. 2. The course of learning in the two groups plotted in terms of mean initial errors per day. Group I learned the simultaneous problem and Group II the successive problem.

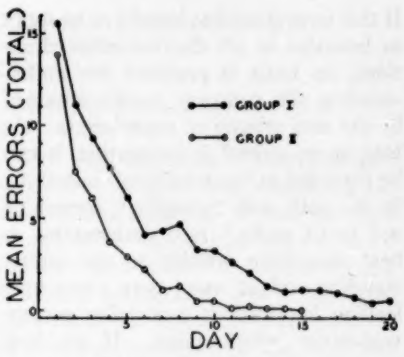

FIG. 3. The course of learning in the two groups plotted in terms of mean total errors per day. Group I learned the simultaneous problem and Group II the successive problem.

When the results of Spence and those obtained with the conventional jumping apparatus are contrasted with the results of Weise and Bitterman and those obtained in the present experiment, the value of a more restricted definition of the concept of *approach* is indicated.

If this term is used so loosely as to apply to behavior in all discriminative situations, no basis is provided for understanding the contrary results obtained in the two groups of experiments. As long as an animal is locomoting, it can be regarded as "approaching" something in its path and "avoiding" something not in its path; [7] such designations at best contribute nothing to our understanding and at worst may carry misleading implications concerning perceptual-motor relationships. If we look toward a more literal definition of approach-avoidance situations (perhaps in terms of the consequences of direct contact with stimulus objects), we have a basis for distinguishing at least two different classes of problem which seem to produce divergent results.

SUMMARY

The Weise-Bitterman experiment on the relative difficulty of simultaneous and successive discrimination has been criticized for complexity of method and failure to control stray light. In the present experiment, which was designed to forestall these criticisms, the Weise-Bitterman results were reproduced with a jumping apparatus. These and other recent experiments suggest the following conclusions: (*a*) in apparatus which require the animal to approach directly (jump at or enter upon) the stimuli to be discriminated, within-pairs differentiation is facilitated and the simultaneous problem is less difficult than the successive; (*b*) in apparatus in which the stimuli are closely juxtaposed and need not be directly approached, configurational organization is facilitated and the simultaneous problem becomes more difficult than the successive; (*c*) the qualitative distinction between relational (within-pairs) and configurational (between-pairs) discrimination continues to be applicable although the question of functional priority cannot be answered in general terms; (*d*) if the concept of approach-avoidance is to be useful, its definition must be considerably restricted.

[7] Nissen (5) has even extended the meaning of the terms to cover the appearance and nonappearance of "tail-twitching."

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FORMALIZATION OF PSYCHOLOGICAL THEORY

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Even though psychologists have been collecting data for over a century, the sizable amount of observational information which they now possess is in a relatively unsystematized state. It is particularly within the last two decades that psychologists have turned to the task of ordering these data. They have come to rather general agreement on the value of the hypothetico-deductive approach to accomplish this task, the first step of which is to choose or build an appropriate formal system. It is with this problem that the present paper is concerned. While we recognize several possible formal systems that can be utilized, it is difficult to say which can most adequately be interpreted in psychology. Only by trial of the more promising ones can an answer be found. Therefore, it behooves us to examine these possibilities and attempt to delimit this domain as well as possible in order that the more promising formal systems may receive the major part of our system construction efforts. This paper, then, will attempt to bring to bear on this problem the knowledge that is now available.

Nature of Formal Systems

A formal system is essentially a set of primitive terms combined to form postulates, from which additional terms and theorems can be derived. It is used in conjunction with (*a*) a set of formation rules which tell under what circumstances a symbol or combination of symbols may be considered to be a permissible expression, e.g., a proposition, and (*b*) a set of transformation rules which state the conditions for operations of deduction.

One type of logical expression with which we are particularly concerned is the synthetic formula. We use this type of expression for the statement of natural laws since it has the unique characteristic of being either true or false, depending on the empirical truth value of substitution instances of the variables employed. It is the prime goal of science to determine which specialized synthetic formulas are true and which are false. One rather widely accepted type of expression of natural laws is the general implication which allows prediction of a second variable, given the first.[1]

Once a scientist chooses a particular formal system to work with he draws from it a set of symbols and relates them to empirical referents. (The formal system is then said to be interpreted.) These symbols are then arranged to form a permissible expression, particularly a synthetic formula, in accordance with the appropriate formation rules. In the process of applying the formal system the scientist must assume that the synthetic statements with which he is working are true. It is not the function of formal systems, and the rules of formation and transformation, to determine which are true and which are false, but their function is exhausted by allowing derivation of new implications from the given synthetic formulas. It is rather the role of statistics to aid in determining the validity of synthetic formulas by determining the relative frequency of true to false instances.

[1] Reichenbach points out some of the difficulties incumbent in this type of expression for natural laws, and the possible solutions to them (9).

NATURAL LANGUAGE

Psychology is now primarily in the stage of stating hypotheses in everyday language, and psychologists are becoming increasingly aware of the limitations of this procedure. It is a healthy commonplace to point out some of the defects of the scientific usage of natural language which can largely be overcome by using any formal system here discussed:

1. It generates ambiguity of the meaning of terms and hypotheses.
2. It does not allow easy translation of concepts between different theories.
3. It does not assure that implications of a theory follow in a rigorous fashion.
4. It provides only subjective criteria for deciding when a proposition is meaningful.
5. It does not sufficiently make accessible all of the implications contained in a hypothesis.
6. It makes impossible the evaluation of a theory according to the standard criteria of completeness, independence, and consistency which, while now not important, will become so when psychology reaches a more advanced stage of development.

Let us, then, briefly examine some of the more promising formal systems.

LOGICAL SYSTEMS

Calculus of propositions. The primitive terms of this calculus include: (*a*) logical operations, "$-$," "$\vee$," "$\cdot$," "$\supset$," "$\equiv$," characterized respectively by the words *not, or, and, if . . . then,* and *material equivalence,* and (*b*) propositional variables—symbols which do not stand for any particular proposition but which occupy a place that can be filled by any special proposition and usually characterized by the letters *a, b,* etc.

In order to apply this formal system to psychology the elementary empirical propositions with which the theory is concerned should be coordinated to propositional variables. These elementary propositions, so symbolized, may then be combined to form more complex ones, in conjunction with the logical operations, as the theory maker desires. Such propositions may then be transformed according to the appropriate rules of transformation in order to arrive at deductions. This procedure may be illustrated by specializing the prototype for the expression of natural laws in Table 1 for the calculus of propositions. Thus, if we let *a* stand for the proposition "an animal is hungry" and *b* for "that animal will eat" and join these symbols by the logical operation of material implication, we have the statement of an extremely elementary law which can be interpreted as: "*If* an animal is hungry, *then* that animal will eat."

Calculus of functions. Whereas in the calculus of propositions the propositions are treated as wholes, in this calculus their inner structure is analyzed. The primitive terms here include: (*a*) term variables which hold open a place for things, usually characterized by the letters *x, y,* etc.; (*b*) predicate variables which hold open a place for properties, usually characterized by the letters *f, g,* etc.; (*c*) the logical operations mentioned in the calculus of propositions.

In applying this calculus the scientist must coordinate the things and properties with which his theory deals to either term or predicate variables and combine them in accordance with his formation

TABLE 1

LOGICAL EXPRESSIONS OF NATURAL LAWS

Calculus	Formula	Natural Language Interpretation
Propositions	$a \supset b$	"If a is true, then b is true"
Functions	$(x)[f(x) \supset g(x)]$	"For all x if x is f, then x is g"
Probability	$P[A, B] = p$	"If A is true, then B is probable to the degree p"

rules to form propositions. Taking the same simple example as previously used, and again referring to the appropriate prototype for expression of natural laws in Table 1, we will let f stand for "a hungry animal," and g for "will eat." The interpretation of this law in the vernacular would then be: "For all x, if x is a hungry animal, then x will eat."

Calculus of probability. Statements are made in this calculus by joining a known and an unknown class with the use of the probability implication, symbolized as "$\overset{p}{\supset}$." This term is the only new primitive term not found in the calculi previously discussed and is interpreted in the context of the basic abbreviated formula of this calculus, "$a \overset{p}{\supset} b$," as "if a is true, then b is probable to the degree p." In order to facilitate applications of this calculus, solutions for the degree p, etc., the probability implication is abbreviated by using equations of the type indicated in Table 1. It is then possible to utilize the general form of the previous calculi discussed within the "P symbol," e.g., the calculus of functions, thus allowing us to perform the same transformations as in these other calculi. An illustration of our example using this calculus would be to let A stand for "An animal is hungry," and B stand for "that animal will eat"; then we can arrive at the following interpretation: "The probability from the statement 'An animal is hungry' to 'That animal will eat' is equal to p." The value of p can then be determined by computing the relative number of true to total instances of empirical observations.

An additional characteristic of this calculus is its ability to handle numerically quantified data in addition to qualitative data. This is done by enumerating elements of a class that possess numerical properties instead of qualitative properties. For an elaboration of this procedure see Reichenbach (10).

One problem that is made particularly apparent in the application of this calculus concerns the assumption that the sequence of events being studied has a limit of frequency at p. This is supposedly a necessary condition for an admissible interpretation, and yet the calculus must be applied prior to knowing that this assumption is satisfied. The general answer to this problem, as Reichenbach (10) points out, is that we do not know for sure that we have chosen a sequence that so converges, but the only chance for success is to proceed as if it does. The criterion of our success is the adequacy of our formulation for making successful predictions. More particularly, we obtain the initial section of a sequence and assume that the value of p thus calculated will persist, within certain limits of exactness, for the rest of the sequence.

Evaluation of logical systems. Since the calculi of propositions and functions are two-valued systems of logic, i.e., the specialized variables contained in the propositions stated in them must be either completely true or completely false, they suffer various disadvantages. Thus, if a given type of behavior is a function of several factors acting simultaneously, the amount contributed by these factors must be taken into account in order to yield a prediction, yet this cannot be done with these calculi. Similarly, predictions cannot be made about a precise quantitative value of an unknown variable, as can be done in, e.g., an algebraic equation. The calculus of functions, when compared to the calculus of propositions, is generally more fertile for yielding deductions since it allows internal analysis of propositions, instead of treating propositions as wholes. The calculus of probability, when utilizing the calculus of functions

within the "P symbol," however, is generally more fertile than either of the other two calculi since it is capable of handling both qualitative and numerically quantitative data. Thus, not only can this calculus handle amounts of variables entering into a behavioral equation, but it can also be used to predict numerically quantitative values of unknown variables.

The two-valued systems of logic are limited in another very important respect. Thus, if any given empirical proposition is subjected to test and but one instance of negation occurs, regardless of the number of positive instances observed, the proposition should properly be considered false. There is no way to state the relationship of positive to negative instances in these calculi. All synthetic statements, however, have a probability character, which makes the calculus of probability the only known logical system rigorously appropriate for application to the physical world. By its use, probability values can be attached to propositions in terms of positive and negative instances observed.

One particular advantage of any of these calculi over the more classical mathematical systems should be pointed out. That is that they all may be used to perform rigorous transformations of qualitatively stated propositions.

MATHEMATICAL SYSTEMS

While it is rather widely accepted that mathematics is reducible to logic we are making a working distinction between these two disciplines. Further, within the field of mathematics we choose to make an arbitrary distinction between classical mathematical systems and unique mathematical systems at a working level.

Classical mathematical systems. This category is intended to include systems which deal with numerically quantified data such as algebra, trigonometry, integral and differential calculus, etc., the primitive terms of which are mathematical variables (x, y, etc.), mathematical constants (a, b, etc.), and various operations such as addition, subtraction, etc. In utilizing these systems the scientist can avail himself of either of two procedures, depending on the state of knowledge in his area. The first consists of discovering relationships from empirically plotted data and fitting the appropriate mathematical functions to those data. In this case there need not be a specific hypothesis in the sense that the parameters of the equation need not have predetermined interpretations. The scientist may then speculate about the parameters of his equation and coordinate empirical terms to them. If such be the case, these interpreted equations would then represent hypotheses which should be validated by testing them against new data.

The second procedure consists of defining empirical terms, coordinating them to formal symbols, and combining these symbols according to the formation rules appropriate to the system involved. Deductions may then be arrived at by putting the equations through the various mathematical operations, e.g., differentiating an algebraic equation. These equations then represent explicit hypotheses which may be subjected to test by fitting them to appropriate empirical data and determining their goodness of fit. This involves the general equation used and the generality of the mathematical constants involved. It is hoped that both are very general in that they do not vary to any great extent for a variety of experimental situations. It is likely, however, that this generality will be limited, e.g., while the general form of the equation may hold throughout a variety of situations, the constants might be limited by specific

types of learning tasks, measurements taken, experimental situations, etc.

Unique mathematical systems. Mathematics has largely developed according to the needs of one science, physics. It is these particular types of mathematics, i.e., classical mathematics, that scientists are most familiar with, and that, therefore, are assumed to be the proper formal systems to use in psychology. There is no particular reason, however, to think that these types of mathematics are particularly applicable in psychology, and it is likely that new systems will have to be developed or discovered. There are a number of instances in the history of science where empirical sciences were able to make great advances only after singularly appropriate formal systems were made available.

Lewin (6) represents the outstanding attempt to apply a unique mathematical system to psychology, which is essentially a composite of vector geometry, topology, and classical mathematics. He attempted to develop empirical constructs which characterize the life space, coordinate them to topological and vector concepts, and then state the laws governing changes in these properties, e.g., he coordinated a need to the vector concept of a system in tension. One unique characteristic of his attempts at formalization is that he utilized formal systems that could handle qualitative data. The answer to the question concerning adequacy of these formal systems is not unequivocal since considerably more work should be done towards this end. Actually Lewin did not progress much farther in applying these geometries than to use a few surface concepts, but what has been done, e.g., his formalization of Zeigarnik's studies, appears to be a definite advancement.

Other preliminary attempts in this direction that appear promising are the efforts of Estes (3) and Bush and Mosteller (1, 2) to utilize set theory in conjunction with classical mathematics.

DISCUSSION

We have indicated that two-valued logic and classical mathematics, taken separately, are inadequate systems for psychology. There thus appear to be three more promising possibilities for attempts at formalization: (*a*) a combination of two-valued logic and classical mathematics, (*b*) a combination of classical mathematics and a unique type of mathematics that is capable of handling qualitative data, e.g., topology, and (*c*) the calculus of probability.

However, in view of the fact that our laws should take the form of a probability statement, the only really appropriate system seems to be probability logic, since it has this capacity, in addition to all the apparent advantages of the other systems. It is, of course, possible that this calculus can be used in combination with a mathematical system, but the conclusion that at least it should be used is inescapable, and it is likely that it alone can be sufficient. There is, on the other hand, no reason to think that only one formal system will be appropriate in psychology but it may well turn out that, as in physics, several geometries can be interpreted with equal adequacy. However, since probability logic is somewhat more complicated to use than two-valued logic and since it is possible to interpret the statements of one in the other without any great difficulty, it may be that the latter can be used to arrive at approximations.

There are, of course, other logical systems that could have been discussed, e.g., calculus of classes, three-valued logic. However, these possess no apparent advantages over the calculi here presented. Thus, both the calculus of

classes and three-valued logic are generalized in probability logic, and neither offers apparent advantages as techniques for approximations over the calculus of propositions or the calculus of functions.

Some examples of two-valued systems applied in psychology are the works of Hull *et al.* (5), Miller (7), and Fitch and Barry (4). Woodger applies them in biology (11), and Reichenbach (8), in the area of quantum mechanics, makes the first application of three-valued logic.

A note of caution is now in order. We must be aware of the waste of energy that would come with premature formalization; this does not mean, however, that we are not ready for it any place in psychology. We should be discriminating and attempt formalization only in well-developed areas where its success is a real possibility. One problem in formalization is the difficulty of encompassing all the well-established facts in a field. This is another indication that our initial attempts should be made in limited and well-founded fields, e.g., aspects of learning, and not attempt to formalize the whole field of psychology at this time. Through experience we will learn to what extent our limited interpreted systems can be concatenated to provide a unified whole in psychology.

Formalization is probably the major problem facing us today. Our more advanced sciences have shown us that great advances come with increased formalization. This is the trend—it is on us. Furthermore, psychologists are the ones who will have to do most of the work. We cannot rely on logicians and mathematicians, because they do not have the necessary psychological knowledge with which to work, and if they did, psychologists would still have to have a working knowledge of the formal system in order to grasp the real meaning of propositions. Interpretations back into the vernacular fail to convey the full essence of complex hypotheses which are formally stated.

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INHIBITION THEORY AND THE EFFORT VARIABLE[1]

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The present paper analyzes a motor learning problem which seems to be somewhat confused. At the theoretical level, the problem is concerned with the Hullian constructs of reactive inhibition and conditioned inhibition (**12**, pp. 277–303). At the empirical level, it consists of determining functional relationships between the effortfulness of the task and certain measures of task performance.

The significance of the effort-inhibition problem stems from the theoretical orientation of much recent motor learning research. A good deal of this research has been concerned with testing hypotheses deduced from Hull's inhibitory constructs, and has shown that the effects of a number of variables on motor task performance are consistent with expectations from inhibition theory. However, such a satisfying state of affairs does not appear to exist for the effort variable. Several *E*s have made predictions concerning the effects of effort on inhibition (**8**, **10**, **11**). Data obtained by these and other *E*s have failed to support the predictions (**4**, **5**, **8**, **10**, **11**).

The thesis of the present paper is that this effort-inhibition research has been misinterpreted, largely because of difficulties inherent in the theory. Following a brief statement of the basic operations and concepts involved in such research, this thesis will be developed by considering three topics: (*a*) the inhibitory constructs, (*b*) the deductions which can properly be made from them, and (*c*) the implications of past research in the light of these more proper deductions. Finally, suggestions for future research will be advanced.

[1] The author wishes to express his thanks to Drs. Benton J. Underwood and Carl P. Duncan, of Northwestern University, and to Richard B. McHugh, of Iowa State College, for their helpful comments on the manuscript. Thanks are also due to George W. McNelly for the many hours spent in discussing issues involved in this paper. Some of these issues were discussed in a paper, jointly authored with Mr. McNelly, presented at the 1952 meeting of the Midwestern Psychological Association.

Basic Operations and Concepts

Operations. Tasks used in Hullian-oriented motor learning research have included reversed alphabet printing, the pursuit rotor, a block-turning task, a ball-placing task, and a hand-cranking task. The experimental treatment usually accorded *S* is the performance of such a task before and after a single interpolated rest. The basic response measure has been *S*'s rate of motor performance, although the major interest has been in response measures derived from this simple measure (see below). The variables manipulated have included number of prerest responses, degree of distribution of prerest practice, length of interpolated rest, and, of course, effort.

The *E*s have manipulated effort in a number of ways. In the pursuit rotor, Eckstrand attached vertical springs of varying tension to the rotor stylus. The *S* had to work against this spring tension in order to bring the stylus down to the plane of the revolving target (**8**). Helmick, who also used the pursuit rotor, believed that he was manipulating effort in varying the speed of rotation of the target (**11**). In the block-turning

task, an attempt was made to manipulate effort by varying the height of the work surface on which the task rested (10). From previous research (9) it was assumed that high work-surface heights were more effortful than moderate heights. More direct manipulations have been used by Bilodeau in simple motor tasks. In a ball-placing task, balls of varying weights were used (4). In a crank-turning situation, Bilodeau varied the force necessary to turn the crank (5).

Theory. These experimental operations have been largely dictated by the desire to test hypotheses deduced from inhibition theory. The basic notions of the theory are simple. It is essentially, as Kimble has noted (15), a two-factor theory of inhibition. Two inhibitory processes are postulated; both are presumed to depress performance. However, the two kinds of inhibition are differentiated in respect to their permanence. One of these inhibitory processes, reactive inhibition (I_R), is temporary in the sense that it dissipates with time. The other, conditioned inhibition ($_sI_R$), is permanent in that it is a habit, a learned response of not-responding. ($_sI_R$ is of course not absolutely permanent; like other habits, it can be forgotten or unlearned.) These two inhibitory factors are presumed to summate to yield a total inhibitory potential, which operates to depress performance.

Indices of the inhibitory constructs. Such a theory yields deductions couched in inhibition language. However, one does not measure inhibition, one measures motor behavior. Thus it is necessary to determine derived response measures which will serve as indices of I_R and $_sI_R$. Both Ammons (1) and Kimble (15) have suggested such indices. Reminiscence, taken as the extent to which a group's initial postrest performance exceeds its final prerest performance, has been used as an index of I_R. The notion is that reminiscence is due to the dissipation of the I_R generated prior to rest, and that the amount of reminiscence thus reflects the amount of I_R dissipated over rest. The index for $_sI_R$ is more complicated. It is based on the notion of comparing initial postrest performance with the performance expected if prerest practice had been free from both kinds of inhibition. A distributed-practice condition which has received approximately the same amount of practice is customarily used to provide the estimate of inhibition-free performance. Figure 1 illustrates this technique of measuring I_R and $_sI_R$ in an experimental group. The distance A is reminiscence, the I_R index. The distance B, which represents the extent to which the experimental group fails to recover to the level of the distributed group, is the $_sI_R$ index.

Past research. These are the two response measures that have been of major research interest. Of the two, the I_R index has received the most attention. It has been shown, as would be expected from theory, that reminiscence is (*a*) directly proportional to the number of prerest responses (2, 4, 10, 13, 15, 18), (*b*) in-

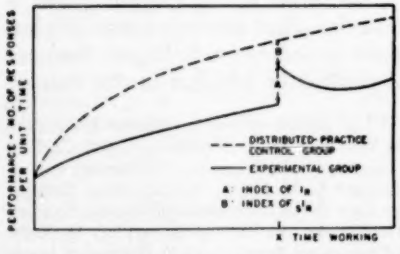

FIG. 1. Indices of reactive inhibition (I_R) and conditioned inhibition ($_sI_R$). A rest is interpolated at time X for the experimental group. Indices noted estimate the amount of I_R and $_sI_R$ generated prior to rest in the experimental group.

versely proportional to the degree of distribution of prerest practice (**3, 6, 16**), and (*c*) directly proportional to the length of the interpolated rest (**2, 4, 10, 13, 14**).

Results from the few experiments using the ${}_{S}I_{R}$ index present a less clear picture. Substantial amounts of ${}_{S}I_{R}$, which behave as expected from theory, have been found in two studies (**15, 20**). However, a recent study (**19**) suggests that this measured ${}_{S}I_{R}$ may be an artifact of experimental procedure, and several other studies (**3, 16, 18**) find little evidence for ${}_{S}I_{R}$. We shall carry the ${}_{S}I_{R}$ construct in our theoretical analysis, but it should be recognized that the evidence for such a construct is, at best, fragmentary.

As has been indicated previously, results from the effort variable do not appear to fit neatly with the theory. The usual prediction is that the I_R index (reminiscence) is proportional to effort. The usual finding is that the I_R index is independent of effort (**4, 5, 8, 10, 11**). No research has been published to date concerning the effects of effort on the ${}_{S}I_{R}$ index. In a subsequent section it will be shown that this prediction of a proportionality between I_R and effort does not necessarily follow from Hull's inhibition theory.

THE INHIBITORY CONSTRUCTS

Reactive inhibition. In his theoretical formulation, Hull specifies three characteristics of his constructs: (*a*) their psychological status, (*b*) the manipulable stimulus variables which influence them, and (*c*) the response from which they may be inferred. I_R is given motivational status; it is described as a response-avoidance drive. The variables which are explicitly stated to determine the amount of I_R generated by a sequence of responses are two: (*a*) the number of responses evoked, and (*b*) the work involved in the execution of the response. The time elapsed since the cessation of responding is a variable of special importance for I_R. I_R is given the special property of spontaneously dissipating with time. Finally, I_R is presumed to reflect itself in behavior through its postulated characteristic of decreasing the probability that the response will occur.

The relationship between I_R and work specified above deserves further comment on two counts. The first is a matter of terminology. In the present paper we are using the term "effort" to describe essentially what Hull means by "work." A number of considerations dictated this choice. First, after defining work as the product of forces times length, Hull points out that the inhibition associated with a given amount of work is not merely a matter of mechanics, but is partially a function of the strength of the muscle system involved (**12**, p. 279). Thus, predictions must concern themselves with more than just the gross physical work accomplished by a response. Second, work can denote several things: physical work (foot-pounds), the fact of performing a set of duties (I went to work), the repeated execution of a well-practiced response (**19**, p. 555), a subjective experience (this is hard work). Third, the trend among current authors seems to be to employ effort rather than work.

It is of course possible to effect a transposition between effort, as used here, and work, defined as the product of force and length. Most *E*s, although they have described their manipulations in terms of effortfulness, have actually endeavored to manipulate work by varying the force requirements of the task. Eckstrand's variation of spring tension in the pursuit rotor, or Bilodeau's variation of the weight of the balls used in the

ball-placing task are clear examples of this. The work-surface height manipulation is not so direct. For this variable one might speculate that the muscular tension generated at high work-surface heights represents an opposing force which has to be overcome in responding. Helmick's variation of the rate of rotation of the pursuit-rotor target is a manipulation which attempts to vary work by varying length, since the rate increase lengthens the target track per unit time. Actually, both the work-surface height and rotor-rate variables are somewhat suspect as work manipulations. For the work-surface height variable it would have to be shown that variation does influence the force requirements of responding. For the rotor-rate variable, it would be necessary to show that the force exerted is equivalent at the rate values used.

The second general comment refers to the fact that the I_R-work relationship given above is actually a simplification of Hull's theorizing. The equation given by Hull is (**12**, p. 279):

$$\text{(i)} \qquad \dot{I}_R = \frac{cn}{B - W},$$

where c and B are constants, n is the number of responses evoked, and W is work.

Confusion attends the $\dot{I}_R$ term. In later portions of the section on inhibition, $\dot{I}_R$ is defined as the total inhibitory potential according to the equation (**12**, p. 285):

$$\text{(ii)} \qquad \dot{I}_R = I_R + {}_sI_R.$$

If equation (i) is taken as stands, then it allows no separation of the effects of W on I_R and ${}_sI_R$. W could influence either or both of the two inhibitory components.

The simplification lies in assuming that Hull was referring to I_R, not $\dot{I}_R$, in equation (i). There are two reasons for such an assumption. First, the equation occurs in the section discussing the characteristics of I_R. Second, this simplification has been made implicitly by a number of previous authors. For example, Kimble, who was one of the first to apply Hullian inhibitory concepts to motor learning problems, has stated that I_R is proportional to work or effort (**14**, p. 239; **16**, p. 500).

Conditioned inhibition. ${}_sI_R$ is given the status of a habit. The reinforcement for this habit is presumed to be the dissipation of I_R which accompanies the cessation of work. When work ceases, I_R dissipates and provides the reinforcing state of affairs necessary to condition the cease-work response to stimuli present. The stimulus variables which influence ${}_sI_R$ are not explicitly stated. However, it seems apparent that the amount of ${}_sI_R$ developed will be primarily a direct function of the amount of I_R present and the rapidity with which it dissipates. Thus, the amount of ${}_sI_R$ generated will hinge on the variables previously discussed which determine the amount of I_R present. Finally, like I_R, ${}_sI_R$ is presumed to decrease the probability of recurrence of the response evoked.

DEDUCTION OF THE EFFECTS OF EFFORT ON THE INHIBITORY CONSTRUCTS

Reactive inhibition. It would seem, from the theory sketched above, that an experimental test of the proportionality between I_R and work or task effort would be straightforward. Work could be varied by altering the effortful demands of the task, and I_R could be indexed by the amount of reminiscence shown after the introduction of an interpolated rest. The theoretical expectation would be that I_R and its reminiscence index would be proportional to work. Unfortu-

nately, the theoretical expectation is not strictly correct. The proportionality between I_R and work holds only for a special case.

Let us examine this theoretical difficulty. It stems from the fact that there is another variable besides n (number of responses) and W (work) which determines the amount of I_R generated by a sequence of responses. This variable, rate of responding (r) is *implicit* in the theoretical formulations of Hull.[2] To illustrate the role of r, consider two response sequences which are equivalent in n and W, but which have different r's. If one sequence is evoked at a very slow rate, it would be possible for all the I_R generated by a response to dissipate before the next response occurred. In this case, the amount of I_R remaining at termination would be merely the amount generated by the last or terminal response. This situation can be contrasted with the case where responses are evoked at a rapid rate. Here, I_R could accumulate throughout the sequence, since there would not be sufficient time for it to dissipate appreciably between responses. In this case the amount of I_R present at termination would be the amount generated by the terminal responses *plus* the undissipated I_R remaining from prior responses.

With the rate variable explicit, the theory takes on quite a different appearance. In essence, it states the following three functional relationships:

(iii) $I_R = g(n)_{Wr}$

(iv) $I_R = h(W)_{nr}$

(v) $I_R = i(r)_{nW}$

where g, h, and i denote functional relationships (I_R increasing with increases in n, W, or r), the subscripts indicate variables which are held constant, and I_R refers to amount of reactive inhibition generated by the response sequence.

While these equations are useful when one is content to vary one response sequence characteristic at a time, additional equations would be needed to handle cases where more than one characteristic varied. Equation (i) is such an additional equation; it handles cases where both n and W vary but r remains constant. However, a final relationship, specifying either the functional relationship of n to W or r to W is not given. Thus, the theory does not handle the general case. It does not permit predictions concerning the relative amount of I_R generated by sequences which differ in W, n, and r. Some of the effort-inhibition research has been of this nature. The *S*s have worked for a given period of time at one of several conditions of effortfulness. The effortful manipulation has depressed rate of responding and thus also altered the number of responses evoked (**4**, **5**, **11**).

However, although it does not follow for the general case, the usual prediction of a proportionality between I_R and work or effort does hold for a special case. From equation (iv) it can be seen that the proportionality follows from theory if response sequences varying in W, but identical in n and r, are compared. This would occur, for example, if the effortful manipulation did not depress rate of responding, and if the groups of *S*s at the various effort conditions all worked for the same length of time. Some of the effort-inhibition research has been of this nature (**8**, **10**). For example, in the work-surface height study (**10**), *S*s working at high (presumably more effortful) heights re-

[2] In fairness to Kimble, it should be pointed out that he has given the rate variable explicit status (**14**, p. 239). However, Kimble has not dealt with the effort variable as such, and thus the rate variable apparently has remained implicit for most *E*s working on the effort-inhibition problem.

sponded just as rapidly prior to rest as *S*s who worked at moderate heights. Since both groups worked for the same length of time prior to rest they were equivalent in total number of prerest responses, and the study thus compares response sequences equivalent in n and r, but supposedly different in effortfulness or work. It will be noted subsequently (see implications of past research) that, while the failure of the effortful manipulation to depress rate of responding rescues the predictions of such experiments, it also raises some doubt as to the adequacy of the effortful manipulation.

Conditioned inhibition. The comments regarding the inability of inhibition theory to handle the I_R-effort function for the general case also have implications for the ${}_sI_R$-effort function. Since ${}_sI_R$ depends for its development on the dissipation of I_R, the theory does not permit predictions in the general case concerning the effects of effort on ${}_sI_R$.

DEDUCTIONS FROM MODIFIED HULLIAN THEORY

Although the theory, as it stands, is indeterminate, it can be coupled with a supporting assumption to yield predictions. This supporting assumption can be seen most clearly within the framework of Kimble's extension of Hull's theory. Kimble adds the notion that organisms will only tolerate a threshold or critical amount of I_R, and that when this threshold is reached the organism rests. For Kimble, this rest may actually occur during the response sequence. Here is Kimble's picture of an organism working under relatively massed practice (**15**, p. 16):

> Since I_R is a drive, it seems only reasonable to suppose that the accumulation of a certain critical amount will automatically produce resting. . . . Presumably, once I_R is reduced to below the critical level, the organism driven by motivation to perform the task at hand will resume work and continue working until the critical level of I_R is reached again. Then it will rest, reducing I_R; start work again, increasing I_R and so on.

Now the supporting assumption is merely that this critical level of I_R is the same for all conditions of effort. Thus, in an effort-reminiscence experiment, the assumption would be that *S*s in the various effort conditions would tolerate approximately equal amounts of I_R. In terms of Kimble's theory, which assumes (**16**, p. 500), with some empirical basis (**17**, **20**), that motivation is the primary determinant of the critical I_R level, this amounts to assuming that the *S*s are equally motivated at the various effort conditions. Or, in alternative terms, one might characterize the assumption as stating that *S*s in the various effort conditions are allotting the same amount of energy to the task at hand. Intuitively, this seems a reasonable assumption. It also has the virtue, as will be seen subsequently, of yielding predictions which are in accord with experimental results.

Reactive inhibition. Figure 2 sketches the development of I_R at two conditions of effort under the modified theory. Consider the curve for the

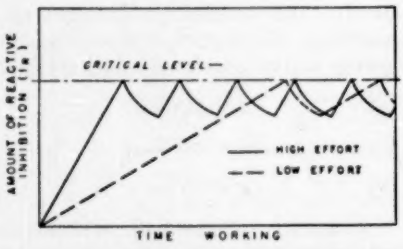

FIG. 2. Theoretical generation of reactive inhibition (I_R) at two conditions of task effort. The curves shown assume a slower rate of responding at the high effort condition than at the low effort condition.

low effort condition. As responses are evoked, I_R cumulates until the critical level is reached. When the amount cumulated reaches this level, the organism stops work, and I_R dissipates. After sufficient I_R has dissipated, the organism starts work again and the cycle is repeated.

The curve for the high effort condition portrays the same sequence of events. Since we have not assumed any particular relationship between task effort and rate of responding, the curve shown is just one of a family of possible high effort curves, each curve corresponding to a particular rate of responding. The particular curve shown rests on the assumption that rate of responding is somewhat depressed at the high effort condition. There are other possibilities. If the effort conditions were identical in rate of responding, the high effort curve would rise even more steeply. If high effort *S*s adjusted their rate of responding so that they were generating the same amount of I_R per unit time as the low effort *S*s, the high and low effort curves would coincide. Finally, if high effort *S*s responded so slowly that they were generating less I_R per unit time than low effort *S*s, the high effort curve would fall below the low effort curve.

The major point of the figure lies in what happens once the two conditions reach the critical level. Irrespective of just what rate of responding adjustment high effort *S*s have adopted, once the critical level is reached the effort conditions should be equivalent in I_R. The I_R-work prediction from the modified theory is thus: *after a moderate amount of practice, I_R is independent of effort.*

Some question naturally arises concerning what is meant by a moderate amount of practice. In his theory, Kimble points out that I_R probably rises quickly to a maximum. If reminiscence is taken as an index of I_R, then empirical amount practice-reminiscence functions furnish estimates of how much time is required for I_R to reach a maximum. Numerous studies (**4, 10, 13, 14**) show that the balance of the growth of I_R is accomplished in the first 3–5 min. practice.

Conditioned inhibition. Figure 2 also permits some comment concerning the effects of effort on ${}_sI_R$. It will be recalled that, in Kimble's theory, it is the resting responses pictured which develop ${}_sI_R$. Figure 2 shows that both effort conditions have identical amounts of I_R present when rest is taken. This, therefore, would not produce differential amounts of ${}_sI_R$ at the two conditions. However, Fig. 2 also points up the possibility that the conditions might differ in the number of resting responses made. This could produce a difference in ${}_sI_R$: the condition with the greater number of resting responses should develop the most ${}_sI_R$.

Whether or not the conditions will differ in number of resting responses and ${}_sI_R$ cannot be predicted from the theoretical formulation given, since it will depend on the kind of rate adjustment adopted by high effort *S*s. For example, if the adjustment shown in Fig. 2 is assumed, there would be more ${}_sI_R$ (more resting responses) at the high effort condition. An alternative assumption is that *S*s adjust their rate of responding to different values of effort so that they are generating comparable amounts of I_R per unit time. This assumption stretches our original equality-of-motivation assumption, which was restricted to points in practice after reaching the critical level, over the entire work period. It would place the curves of Fig. 2 together and yield the prediction that both I_R and ${}_sI_R$ are inde-

pendent of task effort at all stages of practice.[3]

IMPLICATIONS OF PAST RESEARCH

With predictions concerning the effects of effort on I_R and ${}_sI_R$ clarified, let us consider the results of the five relevant studies. At the outset it should be noted that none of these studies has been directly concerned with ${}_sI_R$, so that the discussion will be limited to the I_R construct and its index, reminiscence. The studies may be divided into two groups: (*a*) those in which the effortful manipulation did not depress prerest performance, and (*b*) those in which the effortful manipulation depressed prerest performance.

The Eckstrand (**8**) and the Ellis, Montgomery, and Underwood (**10**) studies belong in the first group. Since prerest rate of responding was not depressed in either study, the correct prediction, according to the previous discussion (see deduction of task effort effects, special case), would be that I_R and reminiscence would be proportional to task effort. In contradiction to this theoretical expectation, both studies found that reminiscence was comparable at the various effortful conditions.

However, as has been pointed out (**10**), there is a consideration which may vitiate the theoretical implications of these studies. Since there appears to be no case in the literature where equal prerest performance is followed by unequal amounts of reminiscence, one might expect that effortful conditions with identical prerest performance would show identical reminiscence.

The Helmick (**11**) and Bilodeau (**4**, **5**) studies belong to the second class of studies. In these studies the effortful manipulations did depress prerest performance. For this case, the prediction from our theoretical section would be that I_R and reminiscence would be independent of effort. This is essentially the finding of the studies. There is some tendency for reminiscence to increase slightly with increases in effort, but there is no statistical evidence that this trend is significant. Thus, it appears that results from contemporary effort-inhibition research are consistent with a slightly modified version of Hullian inhibition theory.

FUTURE RESEARCH

The remedy for those studies in which the effortful manipulation failed to depress prerest performance seems simple. Choose an effortful manipulation and a response measure so that the response measure is sensitive to the effortful manipulation. Actually, this remedy is not so simple. It appears difficult to find response measures which are sensitive to certain types of effortful manipulations. Consider the work-surface height variable. The *S*s working at heights approximately on a level with their shoulders emit signs of effort, such as profanity and requests to cease work, but persistently refuse to satisfy E with depressed performance. Or consider Eckstrand's variation of the spring tension on the pursuit-rotor stylus. At the greatest spring tension, evidence indicated that *S*s had difficulty in working for the required time. Yet, performance was not depressed under this condition.

These observations suggest that there may be two general classes of effortful manipulations, and that the

[3] Bilodeau, in handling the results of one of his studies, has recently arrived at a view generally similar to that presented in this section on deductions from modified Hullian theory. Bilodeau suggests that the *S*s in the various effortful conditions were about equally fatigued, and that they had achieved this state of affairs through adjustments in their rate of responding (**5**, p. 99).

kind of response measure which will be sensitive will depend on what class of effortful manipulation is being used. The two classes may be called response-dependent and response-independent effort.

Work-surface height and stylus spring tension appear to be good examples of response-independent effort because the effort is largely independent of response events. For work-surface height the feature which appears to generate effort is not turning blocks over, but holding arms up. Stylus spring tension has the same general characteristic: the effort exists independently of whether *S* is on or off target. Ordinary performance measures may not be sensitive to such response-independent effortful manipulations. What may be needed is some index of how long *S* will work voluntarily at the task (7). Studies of the influence of task effort on reminiscence scores based on such an index would determine whether the Hullian constructs are useful in handling phenomenon associated with response-independent effort.

As examples of response-dependent effort, Bilodeau's ball-weight and crank-force manipulations can be cited. Here the generation of effort is directly contingent on the evocation of the response measured. Variation of the weight of the blocks in the block-turning task is another example of response-dependent effort. Such a manipulation for the pursuit rotor is harder to envisage. Perhaps an apparatus which provided an increasing gradient of spring tension as the stylus approached the target would be the answer. Our point would be that, for such response-dependent effortful manipulations, ordinary performance measures would be appropriate.

The need for some principle of motivation or energy allotment, so apparent in the theoretical section of this paper, also suggests a fresh approach to some motor learning problems. Specifically, two types of problems may be suggested.

First, although our analysis suggests that one of the basic invariances in the effort experiment is the motivation or amount of energy which *S* allots to the task, we have no guarantee that this invariance will stand up at the extremes of the effort dimension. It may be that present research has been limited to effortful manipulations which cluster at the high or low end of the effort dimension, and that a more adequate exploration of the dimension would show up some systematic relationship between effortfulness and inhibition. Certainly it would appear that some research should be directed at determining features of situations which upset *S*s' usual methods of adjusting energy output.

A second type of problem involves the direct investigation of the brief inhibition-dissipating resting responses postulated by Kimble. The study of long sequences of responses evoked under massed practice should provide information as to the presence of these resting responses and their role in the mechanics of inhibition, and also suggest the laws which govern their appearance. The scheme of energy allotment which *S* achieves with these responses may well be a potent variable in motor learning situations.

SUMMARY

The present paper has dealt with the implications of effort-reminiscence research for Hullian inhibition theory. In the past, such research has been viewed as yielding results inconsistent with this theory.

It is pointed out here that this research has been misinterpreted. First, it is noted that Hullian theory, as stated, does not permit general predictions concerning the effects of

effort on reminiscence. A prediction is presented for the special case where effort does not depress performance, but it is pointed out that such an effortful manipulation does not permit critical tests of the theory. Second, it is shown that, if Hullian inhibition theory is coupled with supporting assumptions concerning how *Ss* allot energy to the task at hand, a prediction of independence between reminiscence and effort can be made. Third, since this is the result obtained by past studies, it is concluded that past research on the reminiscence-effort function is indeed consistent with a modified form of Hullian inhibition theory. Finally, research designed to capitalize on the energy allotment problem made explicit in the theoretical section of the paper is suggested.

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AN APPROACH TO THE STUDY OF COMMUNICATIVE ACTS

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This paper points toward the possibility that many of those phenomena of social behavior which have been somewhat loosely assembled under the label of "interaction" can be more adequately studied as communicative acts. It further points to the possibility that, just as the observable forms of certain solids are macroscopic outcomes of molecular structure, so certain observable group properties are predetermined by the conditions and consequences of communicative acts.

The initial assumption is that communication among humans performs the essential function of enabling two or more individuals to maintain simultaneous orientation toward one another as communicators *and* toward objects of communication. After presenting a rationale for this assumption, we shall attempt to show that a set of propositions derived from or consistent with it seems to be supported by empirical findings.

Co-orientation and the A-B-X System

Every communicative act is viewed as a transmission of information, consisting of discriminative stimuli, from a source to a recipient.[1] For present purposes it is assumed that the discriminative stimuli have a discriminable object as referent. Thus in the simplest possible communicative act one person (A) transmits information to another person (B) about something (X). Such an act is symbolized here as AtoBreX.

The term "orientation" is used as equivalent to "attitude" in its more inclusive sense of referring to both cathectic and cognitive tendencies. The phrase "simultaneous orientation" (hereinafter abbreviated to "co-orientation") itself represents an assumption; namely, that A's orientation toward B and toward X are interdependent. A-B-X is therefore regarded as constituting a system. That is, certain definable relationships between A and B, between A and X, and between B and X are all viewed as interdependent. For some purposes the system may be regarded as a phenomenal one within the life space of A or B, for other purposes as an "objective" system including all of the possible relationships as inferred from observations of A's and B's behavior. It is presumed that a given state of the system exists when a given instance of AtoBreX occurs, and that as a result of this occurrence the system undergoes some change (even though the change be regarded as only a reinforcement of the pre-existing state).

The minimal components of the A-B-X system, as schematically illustrated in Fig. 1, are as follows:

1. A's orientation toward X, including both attitude toward X as an object to be approached or avoided (characterized by sign and intensity) and cognitive attributes (beliefs and cognitive structuring).

2. A's orientations toward B, in exactly the same sense. (For purposes of avoiding confusing terms, we shall speak of posi-

[1] This statement is adapted from G. A. Miller's definition: "'information' is used to refer to *the occurrence of one out of a set of alternative discriminative stimuli.* A discriminative stimulus is a stimulus that is arbitrarily, symbolically, associated with some thing (or state, or event, or property) and that enables the stimulated organism to discriminate this thing from others" (9, p. 41).

tive and negative *attraction* toward A or B as persons, and of favorable and unfavorable *attitudes* toward X.)

3. B's orientation toward X.
4. B's orientation toward A.

In order to examine the possible relationships of similarity and difference between A and B, we shall make use of simple dichotomies in regard to these four relationships. That is, with respect to a given X at a given time, A and B will be regarded as cathectically alike (++ or −−) or different (+− or −+) in attitude and in attraction; and as cognitively alike or different. We shall also make use of simple dichotomies of degree—i.e., more alike, less alike. We shall refer to lateral similarities of A's and B's orientations to X as *symmetrical* relationships.

This very simple system is designed to fit two-person communication. In the following discussion these additional limitations will be imposed, for simplicity's sake: (*a*) communicative acts will be treated as verbal ones, in face-to-face situations; (*b*) initiation of the communicative act is considered to be intentional (i.e., such acts are excluded as those which the actor assumes to be unobserved); (*c*) it is assumed that the "message" is received—i.e., that the communicative act is attended to by an intended recipient, though not necessarily with any particular degree of accuracy; and (*d*) A and B are assumed to be group members, characterized by continued association.

The assumption that co-orientation is essential to human life is based upon

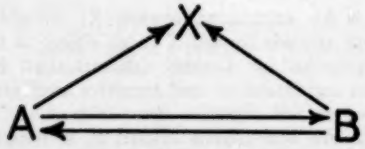

FIG. 1. Schematic illustration of the minimal A-B-X system

two considerations of complementary nature. First, the orientation of any A toward any B (assuming that they are capable of verbal communication) is rarely, if ever, made in an environmental vacuum. Even in what seems the maximally "pure" case of two lovers oblivious to all but each other, both singly and both jointly are dependent upon a common environment; and their continued attachment is notoriously contingent upon the discovery or development of common interests beyond themselves. It is not certain that even their most person-oriented communications (e.g., "I love you") are devoid of environmental reference. The more intense one person's concern for another the more sensitive he is likely to be to the other's orientations to objects in the environment.

Second, the orientation of any A capable of verbal communication about almost any conceivable X is rarely, if ever, made in a social vacuum. There are few if any objects so private that one's orientations toward them are uninfluenced by others' orientations. This is particularly true with regard to what has been termed "social reality" (3); i.e., the less the possibility of testing one's assumptions by observing the physical consequences of those assumptions, the greater the reliance upon social confirmation as the test of what is true and valid. And even when assumptions can be put to the direct test (e.g., the child can find out for himself about the stove which he has been told is hot), social reality is often accepted as the quicker or the safer test. As various linguists have pointed out, moreover, a good deal of social reality is built into the very language with which we communicate about things. Under the conditions of continued association which we are assuming, A and B as they communicate about X are dependent

upon each other, not only because the other's eyes and ears provide an additional source of information about X, but also because the other's judgment provides a testing ground for social reality. And to be dependent upon the other, in so far as such dependence influences behavior, is to be oriented toward him.

In short, it is an almost constant human necessity to orient oneself toward objects in the environment and also toward other persons oriented toward those same objects. To the degree that A's orientation either toward X or toward B is contingent upon B's orientation toward X, A is motivated to influence and/or to inform himself about B's orientation toward X. Communication is the most common and usually the most effective means by which he does so.

SYMMETRY OF ORIENTATION

Much of the remainder of this paper will deal with the relationships between A's and B's orientations toward X, within the postulated A-B-X system. The implications of this model are: (*a*) that while at any given moment the system may be conceived of as being "at rest," it is characterized not by the absence but by the balance of forces; and (*b*) that a change in any part of the system (any of the four relationships portrayed in Fig. 1) may lead to changes in any of the others. We shall also make the assumption (not inherent in the model) that certain forces impinging upon the system are relatively strong and persistent, and that thus there are "strains" toward preferred states of equilibrium.

This assumption, related to the initial one concerning the co-orientation function of communication, is as follows. To the degree that A's orientation toward X is contingent upon B's orientation toward X, A's co-orientation will be facilitated by similarity of his own and B's orientation toward X. The first advantage of symmetry—particularly of cognitive symmetry—is that of ready calculability of the other's behavior; the more similar A's and B's cognitive orientations, the less the necessity for either of them to "translate" X in terms of the other's orientations, the less the likelihood of failure or error in such "translations," and thus the less difficult and/or the less erroneous the co-orientation of either. Second, there is the advantage of validation of one's own orientation toward X; the more similar A's and B's orientations, either cognitive or cathectic (particularly in the many areas where validation is heavily dependent upon "social reality"), the more confident each of them can be of his own cognitive and evaluative orientations. Co-orientation is of course possible with little or no symmetry, but the facilitative value of symmetry for co-orientation is considerable.

If these advantages are commonly experienced as such, communicative acts resulting in increased symmetry are likely to be rewarded, and symmetry is likely to acquire secondary reward value. This is the basis of our assumption of a persistent "strain toward symmetry," under the conditions noted.

These assumptions may now be brought together in terms of the following inclusive postulate: *The stronger the forces toward A's co-orientation in respect to B and X,* (*a*) *the greater A's strain toward symmetry with B in respect to X*; and (*b*) *the greater the likelihood of increased symmetry as a consequence of one or more communicative acts.* The latter part of the postulate assumes the possibility of modified orientations toward X on the part of both A and B, who over a period of time exchange roles as transmitters and receivers of information.

Several testable propositions are derivable from this postulate. First, if the likelihood of instigation to and achievement of symmetry varies as a function of forces toward co-orientation, the latter varies, presumably, with valence of the objects of co-orientation—i.e., of intensity of attitude toward X and of attraction toward B. That is, under conditions such that orientation toward either B or X also demands orientation toward the other, the greater the valence of B or of X the greater the induced force toward co-orientation, and thus the greater the likelihood of both instigation toward and achievement of symmetry.

Such research findings as are known to the writer are in support of these predictions. Experimental results reported by Festinger and Thibaut (5), by Schachter (12), and by Back (1) indicate that attempts to influence another toward one's own point of view vary as a function of attraction. In the second of these studies it is shown that communications within a cohesive group are directed most frequently toward those perceived as deviates, up to a point where the deviate is sociometrically rejected (i.e., attraction decreases or becomes negative), beyond which point communication to them becomes less frequent. It is also shown in this study that frequency of influence-attempting communication varies with degree of interest in the topic of group discussion.

Some of these same studies, and some others, present data concerning symmetry as a consequence of communication. Thus Festinger and Thibaut, varying "pressure toward uniformity" and "perception of homogeneous group composition," found actual change toward uniformity following a discussion to be a function of both these variables, but some change toward uniformity took place in every group, under all conditions. Back found that subjects who started with different interpretations of the same material and who were given an opportunity to discuss the matter were influenced by each other as a direct function of attraction.

Findings from two community studies may also be cited, as consistent with these laboratory studies. Newcomb (10), in a replicated study of friendship choices as related to political attitudes in a small college community, found on both occasions that students at each extreme of the attitude continuum tended to have as friends those like themselves in attitude. Festinger, Schachter, and Back (4), in their study of a housing project, found a correlation of +.72 between a measure of attraction and a measure of "conformity in attitude." No direct observations of communication are made in these two studies; the relevance of their findings for the present point depends upon the assumption that frequency of communication is a function of attraction. This assumption is clearly justified in these two particular investigations, since in both communities there was complete freedom of association. As noted below, this assumption is not justified in all situations.

Other testable propositions derivable from the general postulate have to do with A's judgments of existing symmetry between himself and B with respect to X. Such judgments (to which the writer has for some time applied the term "perceived consensus") are represented by the symbol *B-X*, within A's phenomenal A-B-X system. Such a judgment, under given conditions of demand for co-orientation with respect to a given B and a given X, is a major determinant of the likelihood of a given AtoBreX, since strain toward symmetry is influenced by perception of existing symmetry. Such a judgment, more-

over, is either confirmed or modified by whatever response B makes to AtoBreX. The continuity of an A-B-X system thus depends upon perceived consensus, which may be viewed either as an independent or as a dependent variable.

According to the previous proposition, the likelihood of increased symmetry (objectively observed) as a consequence of communicative acts increases with attraction and with intensity of attitude. The likelihood of perceived symmetry presumably increases with the same variables. Judgments of symmetry, like other judgments, are influenced both by "reality" and by "autistic" factors, both of which tend, as a function of attraction and intensity of attitude, to increase the likelihood of perceived consensus. Frequency of communication with B about X is the most important of the "reality" factors, and this, as we have seen, tends to vary with valence toward B and toward X. As for the "autistic" factors, the greater the positive attraction toward B and the more intense the attitude toward X, the greater the likelihood of cognitive distortion toward symmetry. Hypothetically, then, perceived symmetry with regard to X varies as a function of intensity of attitude toward X and of attraction toward B.

A considerable number of research studies, published and unpublished, are known to the writer in which subjects' own attitudes are related to their estimates of majority or modal position of specified groups. Only a minority of the studies systematically relate these judgments to attraction, and still fewer to intensity of attitude. Among this minority, however, the writer knows of no exceptions to the above proposition. The most striking of the known findings were obtained from students in several university classes in April of 1951, in a questionnaire dealing with the very recent dismissal of General MacArthur by President Truman:

	pro-Truman *Ss* who . . .	anti-Truman *Ss* who . . .
attribute to "most of my closest friends"		
pro-Truman attitudes	48	2
anti-Truman attitudes	0	34
neither	4	4
attribute to "most uninformed people"		
pro-Truman attitudes	6	13
anti-Truman attitudes	32	14
neither	14	13

If we assume that "closest friends" are more attractive to university students than "uninformed people," these data provide support for the attraction hypothesis. Comparisons of those whose own attitudes are more and less intense also provide support, though less strikingly, for the hypothesis concerning attitude intensity.

Perceived symmetry, viewed as an independent variable, is obviously a determinant of instigation to symmetry-directed communication. Festinger (3), with specific reference to groups characterized by "pressures toward uniformity," hypothesizes that "pressure on members to communicate to others in the group concerning item x increases monotonically with increase in the perceived discrepancy in opinion concerning item x among members of the group," as well as with "relevance of item x to the functioning of the group," and with "cohesiveness of the group." And, with reference to the choice of recipient for communications, "The force to communicate about item x to a particular member of the group will increase as the discrepancy in opinion

between that member and the communicator increases [and] will decrease to the extent that he is perceived as not a member of the group or to the extent that he is not wanted as a member of the group" (3, p. 8). Support for all of these hypotheses is to be found in one or more of his and his associates' studies. They are consistent with the following proposition: the likelihood of a symmetry-directed AtoBreX varies as a multiple function of perceived discrepancy (i.e., inversely with perceived symmetry), with valence toward B and with valence toward X.

Common sense and selected observations from everyday behavior may also be adduced in support of these propositions. For example, A observes that an attractive B differs with him on an important issue and seeks symmetry by trying to persuade B to his own point of view; or A seeks to reassure himself that B does not disagree with him; or A gives information to B about X or asks B for information about X. From all these acts we may infer perception of asymmetry and direction of communication toward symmetry. Selected observations concerning symmetry as a consequence of communication are equally plentiful; there is, in fact, no social phenomenon which can be more commonly observed than the tendency for freely communicating persons to resemble one another in orientation toward objects of common concern. The very nature of the communicative act as a transmission of information would, on a priori grounds alone, lead to the prediction of increased symmetry, since following the communication both A and B possess the information which was only A's before. B will not necessarily accept or believe all information transmitted by A, of course, but the likelihood of his doing so presumably varies not only with attraction toward A but also with intensity of attitude toward X, since in the long run the more important X is to him the more likely it is that he will avoid communicating with B about X if he cannot believe him. Thus the propositions have a considerable degree of face validity.

But everyday observation also provides instances to the contrary. Not all communications are directed toward symmetry, nor is symmetry an inevitable consequence of communication, even when attraction is strong and attitudes are intense. A devoted husband may refrain from discussing important business matters with his wife, or two close friends may "agree to disagree" in silence about matters of importance to both. People who are attracted toward one another often continue to communicate about subjects on which they continue to disagree—and this is particularly apt to happen with regard to attitudes which are intense, contrary to our theoretical prediction.

In sum, the available research findings and a considerable body of everyday observation support our predictions that instigation toward, perception of, and actual achievement of symmetry vary with intensity of attitude toward X and attraction toward B. The readiness with which exceptions can be adduced, however, indicates that these are not the only variables involved. The propositions, at best, rest upon the assumption of *ceteris paribus;* they cannot account for the fact that the probabilities of A's instigation to communicate about a given X are not the same for all potential B's of equal attraction for him, nor the fact that his instigation to communicate to a given B are not the same for all X's of equal valence to him. We shall therefore attempt to derive certain further propositions from our basic assumption that both instigation to and achievement of symmetry vary with strength of forces toward coorientation in the given situation.

DYNAMICS OF CO-ORIENTATION

The foregoing propositions represent only a slight extrapolation of Heider's general principle (6) of "balanced states" in the absence of which "unit relations will be changed through action or through cognitive reorganization." In a later paper devoted specifically to the implications of Heider's hypotheses for interrelationships among attitudes toward a person and toward his acts, Horowitz *et al.* (8) note the following possible resolutions to states of imbalance: (*a*) the sign-valence of the act is changed to agree with that of the actor; (*b*) the reverse of this; and (*c*) the act is cognitively divorced from the actor; in addition, of course, the disharmony may be tolerated.

Orientations as attributed by A to B are here considered as equivalent to acts so attributed, in Heider's sense, and symmetry is taken as a special case of balance. Assume, for example, the following asymmetry in A's phenomenal system: +A:X, +A:B, $-B{:}X$, $+B{:}A$ (i.e., A has positive attitude toward X, positive attraction toward B, perceives B's attitude toward X as negative, and B's attraction toward A as positive). Any of the following attempts at "resolution," analogous to those mentioned by Heider, are possible: (*a*) −A:X; (*b*) −A:B; or (*c*) cognitive dissociation. These can occur in the absence of any communication with B. Attempts at harmony (symmetry) may also be made via communications directed toward $+B{:}X$. And, if such attempts fail, the three alternatives mentioned as possible without communication are still available. Finally, there is the possibility of compromise, following communication (e.g., agreement on some midpoint), and the possibility of "agreeing to disagree."

Such acts of resolution are made necessary, according to the present theory, by the situational demands of co-orientation on the one hand and by the psychological strain toward symmetry on the other. But symmetry is only a facilitating condition for co-orientation, not a necessary one. While (as maintained in the preceding propositions) the probabilities of symmetry vary, *ceteris paribus*, with demand for co-orientation, the theory does not demand that a symmetry-directed AtoBreX occur in every instance of strong demand for co-orientation. On the contrary, the theory demands that it occur only if, as, and when co-orientation is facilitated thereby. We must therefore inquire more closely into the nature of the forces toward co-orientation as related to possible forces against symmetry.

One kind of situational variable has to do with the nature of the forces which result in association between A and B. Of particular importance are constrained (enforced) vs. voluntary association, and association based upon broad as contrasted with narrow common interests. The range of X's with regard to which there is demand for co-orientation is presumably influenced by such forces. The relevant generalization seems to be as follows: *The less the attraction between A and B, the more nearly strain toward symmetry is limited to those particular X's co-orientation toward which is required by the conditions of association.* This would mean, for example, that as attraction between two spouses decreases, strain toward symmetry would increasingly narrow to such X's as are required by personal comfort and conformity with external propriety; similarly, the range of X's with regard to which there is strain toward symmetry is greater for two friendly than for two hostile members of a chess club.

The problem of constraint has already been noted. In some of the stud-

ies cited above it was assumed that frequency of communication varies with attraction, but this is not necessarily true under conditions of forced association. Two recent theoretical treatises deal with this problem.

Homans, one of whose group variables is "frequency of interaction" (though not communication, specifically), includes the following among his other propositions: "If the frequency of interaction between two or more persons increases, the degree of their liking for one another will increase, and vice versa"; and "The more frequently persons interact with one another, the more alike in some respects both their activities and their sentiments tend to become" (7, p. 120). (The latter proposition, which closely resembles the one here under consideration, apparently takes a much less important place in Homans' system than the former.) Almost immediately, however, the latter proposition is qualified by the statement, "It is only when people interact as social equals and their jobs are not sharply differentiated that our hypothesis comes fully into its own." In nearly every chapter, moreover, Homans (whose propositions are drawn *post hoc* from various community, industrial, and ethnological studies) points to the limitations which are imposed by constraining forces—particularly those of rank and hierarchy—upon the relations among attraction, similarity of attitude, and communication.

Blake manages to incorporate these considerations in a more rigorous proposition. Noting that hostility cannot be considered as the simple psychological opposite of positive attraction, he proposes to substitute a curvilinear for Homans' linear hypothesis: ". . . when pressures operate to keep members of a group together, the stresses that drive toward interaction will be stronger in ***both*** positive and negative feeling states than in neutral ones" (2). This proposition seems consistent with the present argument to the effect that demands for co-orientation are likely to vary with the nature and degree of constraints upon association; hence communicative acts, together with their consequences, will also vary with such constraints.

Another situational variable deals with the fact that, under conditions of prescribed role differentiation, symmetry may take the form of "complementarity" (cf. 11) rather than sameness. For example, both a man and his small son may (following a certain amount of communication of a certain nature) subscribe to the *same norms* which prescribe *differentiated behavior* for man and boy with respect to a whiskey and soda. If the father drinks in the son's presence, there are demands upon both of them for co-orientation; but there is strain toward symmetry only with respect to "the code," and not with respect to personal orientation toward the whiskey and soda. The code becomes the X with regard to which there is strain toward symmetry. In more general terms, *under conditions of differentiation of A's and B's role prescriptions with regard to X, the greater the demand for co-orientation the greater the likelihood of strain toward symmetry with respect to the role system* (rather than with respect to X itself).

A third situational variable has to do with the possibility that symmetry may be threatening. Particularly under circumstances of shame, guilt, or fear of punishment there are apt to be strong forces against a symmetry-directed AtoBreX, even though—in fact, especially when—attitude toward X (the guilty act) and attraction toward B (a person from whom it is to be concealed) are strong. Under these conditions it is the demand for co-orientation which creates the problem; if A could utterly divorce X (his own act) from B, he would not

feel guilty. Forces toward symmetry, however, are opposed by counterforces. Demand for co-orientation induces strain toward symmetry, but does not necessarily lead to a symmetry-directed Ato-BreX.

A theoretically analogous situation may result from the omnipresent fact of multiple membership groups. That is, strains toward symmetry with B_1 in regard to X may be outweighed by strains toward symmetry with B_2, whose orientations toward X are viewed as contradictory with those of B_1. This is often the case when, for example, two good friends "agree to disagree" about something of importance to both. Thus in one study (14) it was found that those members least influenced by reported information concerning their own group norms were those most attracted to groups whose norms were perceived as highly divergent from those of the group in question.

Communicative acts, like others, are thus subject to inhibition. Such "resolutions" as "agreement to disagree," however, represent relatively stressful states of equilibrium. It is therefore to be expected, in ways analogous to those noted by Lewin in his discussion of the quasi-stationary equilibrium, that A-B-X systems characterized by such stress will be particularly susceptible to change. Such change need not necessarily occur in the particular region of the system characterized by maximal strain.

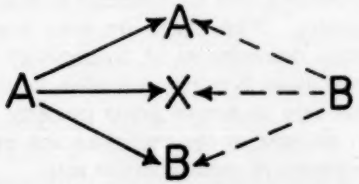

FIG. 2. Schematic illustration of A's phenomenal A-B-X system

The dynamics of such a system are by no means limited to those of strains toward symmetry, but must include changes resulting from acceptance of existing asymmetry. The possible range of dynamic changes is illustrated in Fig. 2. (In this figure, the A and B at either side represent persons as communicators; the A and B in the center represent the same persons as objects of co-orientation. The broken lines represent A's judgments of B's orientations.) Given perceived asymmetry with regard to X, and demand for co-orientation toward B and X, the possibilities for A are such that he can:

1. achieve, or attempt to achieve, symmetry with regard to X
   a. by influencing B toward own orientation,
   b. by changing own orientation toward B's,
   c. by cognitively distorting B's orientation;
2. introduce changes in other parts of the system
   a. modify his attraction toward B,
   b. modify his judgment of own attraction for B,
   c. modify evaluation of (attraction toward) himself (A),
   d. modify his judgment of B's evaluation of himself (B);
3. tolerate the asymmetry, without change.

As suggested by this listing of possible "solutions," the perception of asymmetry, under conditions of demand for co-orientation, confronts A with a problem which he can attempt to solve behaviorally (i.e., by communicative acts) and/or cognitively (i.e., by changing either his own orientations or his perception of B's orientations). Whatever his chosen "solution," it has some effect upon A's phenomenal A-B-X system—either to reinforce it or to modify it. As a result of repeatedly facing and "solving" problems of co-orientation with regard to a given B and a given X,

a relatively stable equilibrium is established. If A is free either to continue or not to continue his association with B, one or the other of two eventual outcomes is likely: (*a*) he achieves an equilibrium characterized by relatively great attraction toward B and by relatively high perceived symmetry, and the association is continued; or (*b*) he achieves an equilibrium characterized by relatively little attraction toward B and by relatively low perceived symmetry, and the association is discontinued. This "either-or" assumption under conditions of low constraint presupposes a circular relationship between attraction and the perception of symmetry. The present theory demands this assumption of circularity, and empirical evidence (under conditions of relative freedom from constraint) seems to support it.

Under conditions of little or no freedom to discontinue association, no such circularity is assumed. The conditions which dictate continued association also dictate the requirements for co-orientation, which are independent of attraction. The empirical data suggest that the degree to which attraction is independent of symmetry varies with the degree of *perceived* (rather than the degree of objectively observed) constraint.

GROUP PROPERTIES

It follows from the preceding assumptions and propositions that there should be predictable relationships between certain properties of any group and variables having to do with communicative behavior within that group. A group's structural properties, for example, viewed as independent variables, may create problems and may provide solutions to other problems of communication. Viewed the other way around, many properties of a group are outcomes of its communicative practices. Evidence from many sources points to distinctive properties of groups which are precisely those which the foregoing considerations would lead us to expect, either as conditions for or as consequences of a given kind and frequency of communicative acts.

Three kinds of properties are briefly noted. Each of them is hypothetically related (either as dependent or as independent variable) to the probabilities of the occurrence of a given kind of communicative act.

1. *Homogeneity of orientation* toward certain objects. All descriptive accounts of interacting groups note this property, in one way or another and by one label or another. As applied to behavior, it does not necessarily refer to similarity of action on the part of all group members, but only of demand or expectation; e.g., all expect each to take his own differentiated role. In order to account for the observed facts it is necessary to make the assumptions (not previously made in this paper) that information may be transmitted in nonverbal ways, and with or without intention to do so—e.g., a person's behavior with regard to a given object informs observers about his orientation to it.

If communication is thus broadly defined, then the degrees of homogeneity of orientation of a given group with respect to specified objects are presumably related to communication variables with respect to those objects. It is not hypothesized that homogeneity is an invariable function of any single index of communication (frequency, for example), but rather that it varies in accordance with the dynamics of A-B-X systems. While there are often extra-group determinants of homogeneity of orientation, it seems reasonable to view this very important group property as an outcome of the conditions and consequences of communicative acts.

2. *Homogeneity of perceived consensus* (i.e., homogeneity of judgments

of homogeneity of orientation). This property, though not often specifically mentioned in the literature on groups, is usually implicitly assumed. Most communication presupposes a considerable degree of perceived as well as objective homogeneity of orientation. The very fact of using language or gesture presupposes the assumption of consensus among communicants as to the information transmitted by the use of symbols.

Homogeneity of orientation and of perceived consensus do not, in spite of implicit assumptions to the contrary, have an invariant relationship; judgments of homogeneity may be of any degree of accuracy. If, as in the village reported by Schanck (13), each of many dissenters from a supposed norm believes himself the only dissenter, this state of pluralistic ignorance is an important group property, and is plausibly described by the author as an outcome of certain practices of communication. Any degree of homogeneity of perceived consensus, representing any degree of accuracy, is hypothetically an outcome of previous communicative acts and a determinant of future ones.

3. *Attraction among members.* Relationships of positive attraction of some degree invariably characterize continuing groups under conditions of minimal constraint, and are commonly found even under conditions of considerable constraint. This is so commonly the case that Homans (7) ventures the almost unqualified hypothesis that "liking" increases with frequency of interaction, and vice versa. Viewed in the light of the hypothetical dynamics of A-B-X systems, Homans' proposition would be amended to the effect that interpersonal attraction varies with the degree to which the demands of co-orientation are met by communicative acts.

These are not, of course, the only group properties of significance, nor are these properties outcomes exclusively of intragroup communication. (Some properties of almost any group, particularly at early stages of its history, derive largely from individual characteristics which its members bring to it.) It appears to be the case, nevertheless, that the hypothetical conditions and consequences of communicative acts are not limited to groups of two, and that some of the important properties of observed groups are consistent with the hypothetical dynamics of A-B-X systems.

SUMMARY

Communicative acts, like other molar behaviors, may be viewed as outcomes of changes in organism-environment relationships, actual and/or anticipated. Communicative acts are distinctive in that they may be aroused by and may result in changes anywhere within the system of relations between two or more communicators and the objects of their communication. It seems likely that the dynamics of such a system are such that from an adequate understanding of its properties at a given moment there can be predicted both the likelihood of occurrence of a given act of communication and the nature of changes in those properties which will result from that act.

Some of the most significant of group properties are those which, hypothetically, vary with intragroup communicative acts. It should therefore be rewarding to discover whether support for the present hypotheses, as apparently provided by the scattered evidence now available, can be confirmed in more systematic ways. If so, there are promising possibilities of investigating the phenomena of social interaction by viewing them as events within communication systems.

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ON SOME CORRUPTIONS OF THE DOCTRINE OF HOMEOSTASIS

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Cannon's doctrine of homeostasis (2) simply asserts that many enduring and apparently static physiological states are in fact the product of a constant opposition of forces—an interchange of substances, a constant adding-to and taking-away—and that with some of these, when there is a departure from the balance then that departure sets in motion processes which (under specifiable conditions) restore the balance. (Although the action of many of these physiological mechanisms was first indicated by Cannon, the discovery of similar "constant inconstancies" had been made many times before, e.g., by Heraclitus, who contended that anything at all was made up of such steady states of flux.)

But in many recent treatments of homeostasis all that is retained is just the steadiness, the tendency to restoration. The constituent opposing processes are lost sight of, and so are the compensating mechanisms, and the suggestion is conveyed more or less explicitly that the mere departure from the condition in question is sufficient to restore that condition—as one might unreflectingly think that the mere deviation of a pendulum from the vertical (as a result of some external force) is sufficient to start it on its way back to the vertical (almost as though there were something "illogical" or "unnatural" about any other position). When we consider the number and variety of places from which any given state—say, a certain temperature or a certain concentration of carbon dioxide—is absent, then it becomes plain that mere absence alone is never sufficient eventually to produce that condition. We must consider what is present in, as well as absent from, the place we are talking about, what structures and processes are there to act on each other and so produce the effect being investigated. Just the same point can be made about doctrines which use the principle of "closure" to explain the emergence of certain perceptions, or motor actions, or neural patterns.

Many instances of these opposing processes and their attendant mechanisms are discussed by Cannon. To take only one example, one relatively constant physiological state is the concentration of carbon dioxide in the blood stream. Carbon dioxide is continually being produced, primarily by the combustion of lactic acid in the working muscles, and continually passing out of the blood through the lungs. When, for any of a number of reasons, one of them—the production, say—outstrips the other, so that the concentration of carbon dioxide rises, this increase has among its many effects that of exciting certain nerve endings in the carotid sinus which produce direct, reflex stimulation of the musculature to produce more vigorous breathing, by which the rate of excretion of carbon dioxide in the lungs is increased, so that, under specifiable conditions, the carbon dioxide concentration falls. It is misleading (although perhaps in fact correct) to say, as is often done, that it falls until it passes back below the "danger point," whereupon the respiration returns to normal. What happens, of course, is that it falls below the threshold at which those nerve endings are excited, so that the extra impulses are no longer delivered to the respiratory muscles, which

naturally then return to their former level of work.

Now, to give such a balancing the name "homeostasis" seems unobjectionable, but the mere existence of the word provides another opportunity for us to give up the tiresome search for those mechanisms which actually produce the effect in question, the constancy, and simply to attribute its production to the activity of a vaguely conceived and therefore very accommodating force named "homeostasis." Nobody would, of course, perpetrate this fallacy in such a crass fashion; it is hard to convict people of it because its very crudity leads them unconsciously to shy away from it and disguise it in ambiguous phrases. Yet as Lashley (5) pointed out, theories of motivation are riddled with such occult powers.

Even Fletcher (4), who professedly wants to exhibit homeostasis as an explanatory principle, says nothing which is unequivocally open to this objection, although one might contend that if he is not speaking of homeostasis as a force which produces constancies, then he is really not saying much at all. For example, concerning the retinal changes in color vision, as sketched by the Hering theory, he says, "We, of course, do not know much about the original organic status, nor precisely how it is disturbed, nor yet concerning the process of recuperation. Yet we cannot but assume the existence of such a status, the disturbance of the status, and a process which brings about its restoration" (4, p. 84). Since he offers this as an example of the use that "can be made of the principle of homeostasis in dealing with some well-known, but as yet not well-explained mental phenomena," it would appear that the "process" which restores the status is just homeostasis itself. But of course homeostasis is simply the *fact* of restoration, the word is only another name for the restoring, and so it cannot be appealed to as something which explains that restoration.

This point appears again when Fletcher refers to the J. J. B. Morgan experiments on the effect of distraction. "The now familiar fact that such distractions do not always decrease, but may even increase performance is taken to indicate a tendency to maintain a certain work level" (4, p. 85). Now, the fact that the subject tends to maintain a certain work level is not what is indicated but just what is *observed*—that is, that he usually *does* maintain it. This alleged explanation then merely restates the fact to be explained.

Homeostasis, then, is to be seen not as the *cause* of "steady states," but as the steadiness itself, as the *effect* of various specific qualitative processes in the organism. Nor can references just to the activity of "the organism" replace the discovery of those specific processes or mechanisms. A proper discussion of this issue is beyond the scope of this essay, but at least one can contend that the opposition between the organismic (or holistic or Gestalt) view and the atomistic view is not, as Wheeler (10) claims, exhaustive of the possibilities. We are not required to see either the whole as primary and prior to the parts or the parts as primary and prior to the whole. The solution is that both whole and parts must be recognized as having distinct, even if not separable, existence. The organismic approach is of course not to be discarded completely; in brief, we must recognize that there *are* wholes having their own characteristics. But an extreme organismic view, holding that no statement which treats the parts as distinct from each other and from the whole can be quite true, is faced with the problem of showing how there can be said to be a whole of parts (in the plural) at all. If the parts are not distinct from each other and from

the whole then they are identical with it, that is, it cannot actually be said to *have* parts, that is, it is indistinguishable from an atom, and Wheeler's disjuncts are not only not exhaustive but not even exclusive.

Statements, then, such as Stagner's that "when a given tissue constancy is first disturbed, the organism mobilizes energy for action which ceases when the equilibrium is restored" (7, p. 5), can be quite misleading, even though they may be intended only as preliminary and liable to further elaboration. This elaboration, if it is ever offered, must show that it is not that the organism mobilizes energy for action, but only that certain parts of it are caused to act in specific ways, either by circumstances external to the organism or by the action on them of other parts of it. There is a special case in which this objection seems not to apply—namely, when we are speaking of psychological actions involving intentions and using knowledge gained in past experience. But the notion that establishing homeostasis is something that "the organism" does is carried over into the field of physiology, and although once again it may itself seem unobjectionable if a trifle vague, it readily leads to the suggestion that organisms possess or develop various mechanisms *in order* to achieve homeostasis. Putting it another way, the notion arises that organisms possess a special kind of causality, namely, a teleological causality, so that the part-processes within an organism are determined not by the *nature* of those parts (since they have no distinct nature), but by the effect which *is to be produced* by them in the organism as a whole.

Anything more than a suggestion of this is rarely found, but nevertheless we do find these suggestions even in Cannon's use of such phrases as "the means employed by the more highly evolved animals for preserving uniform and stable their internal economy" (2, p. 24); and in Dempsey's ". . . in order to free themselves from their marine habitat, they had to devise mechanisms for maintaining the fluid concentrations of the body" (3, p. 230). Again, when Fletcher, speaking of the capacity of the physical organism "to combat invading foreign bodies and pathogenic organisms," says that "the adaptability of this capacity to deal with novel conditions is suggestive of a form of intelligence" (4, p. 80), he is thinking of the physiological events as means adopted for a certain end, rather than as events whose effects happen to have survival value for the organism in question, that is, as long as it happens to remain within a certain kind of environment.

How would we deal, on this view, with the evidence Fletcher refers to that "the introduction of foreign substances into the blood stream of certain animals provoked the formation of antibodies specifically qualified to destroy these substances, even though, presumably, the animals had never been exposed to such substances in the course of their evolution" (4, p. 81)? One point would be that this absence of previous exposure is quite irrelevant; what is relevant is whether they previously possessed any, no matter how few, of the organisms which are now called antibodies. Another important point is that this capacity to form antibodies is highly selective for different foreign substances; otherwise, we could not understand how these animals could ever be poisoned, or die of a contagious disease. What determines this difference, that some foreign substances are destroyed and others not? In the former case there must already be present in the animal something, presumably some kind of organism, for whose propagation the presence of the foreign sub-

stance provides favorable conditions—and how often do we find that "providing favorable conditions" means being consumed by, that is literally being *food* for, the thing in question? There is nothing surprising in the presence of a certain crop or species of game resulting in the increase, in that area, of the human race—organisms "specifically qualified" to destroy that crop or game.

There is nothing in homeostasis which requires a special kind of organic causality, nothing which cannot be rendered in statements of antecedent conditions and consequent effects, nothing, in fact, peculiar to organisms at all. The thermostat is often used as a physical analogy. An even more primitive but most effective homeostatic mechanism is the outrigger of a canoe. As the canoe begins to tip over to one side, because of the action of the waves, for example, the float of the outrigger begins to rise in the water, its weight increases, and this swings the canoe back. If it goes too far, the float is pressed down into the water, the upward pressure on it is increased, and again there is a return toward the even keel. These mechanisms, of course, happen to be arranged by men, but no foreseeing of ends is necessary for homeostasis; any floating log with a projecting branch exhibits just the same behavior as that indicated in the canoe with its outrigger.

One point arising in each of these examples makes it plain that homeostasis is not just one process, and so that it cannot be dismissed just with a reference to "the organism." That is, there are always at least two mechanisms, two causal sequences, involved: if two forces A and B are opposing each other in a homeostatic situation, then one mechanism or process will be brought into play when A outstrips B and a different (although often intimately related) one when B outstrips A. These may be processes internal to A or B rather than something opposing them externally; e.g., it might simply be that A can only stretch out, as it were, to a certain extent and then becomes exhausted—a principle which seems to be clearly enough recognized in warfare, supposing A now to be an advancing army. This internality may give the impression that homeostasis *is* a force in itself, that there *is* something illogical about the departure from balance, but actually we can only understand this internal necessity by realizing that A (*whatever* it may be) is articulated, has distinct parts, and that the "internal necessity" is an external action of those parts on one another. In this instance we might have to take notice, e.g., of a lack of coordination between the general staff dictating the speed of advance and the supply corps trying to keep up the materials consumed by that advance.

Now, a further important point, and one which in my opinion renders otiose many "uses" of the principle of homeostasis, is that what is restored is not just "equilibrium," as in the phrase I quoted from Stagner, but some specific amount or concentration or intensity of a particular property. That is, what we have to recognize is not just equilibrium or constancy, but *kind*. Thus the outrigger keeps the canoe not just steady but right-side-up; the thermostat maintains not just any steady temperature but one within a specified range. If it were possible for the many relevant mechanisms to stabilize the pH of the blood at a point outside the range 6.8 to 7.8, then death would speedily ensue. Plainly in this case, if we were interested in the organism's future, it would be of crucial importance to know not merely that the pH was constant, but at what point it was constant.

Stagner, in the article in question

(7), and in his recent text with Karwoski (8), seems to have become the chief exponent of homeostasis as a foundation for psychological theorizing, and, I would contend, the leader in concentrating on constancy and neglecting to observe what the constant states are. In proposing homeostasis not as an explanatory principle, but just as a "unifying concept," he adopts the position that all goal-directed behavior originates in the endeavor to maintain tissue constancies. After some experience the organism anticipates disturbance, and "perceives environmental objects as potential sources of equilibrium-restoration" (7, p. 5). One of the means-activities directed towards this end is the adoption of perceptual constancies. "Objects, as stimuli affecting the distance receptors, are protean in size, shape, and color. Under such conditions adjustment is most difficult. The organism therefore *learns* to perceive identical objects as possessing these constant attributes" (7, p. 7).

Concerning the size, shape, and color constancies, it seems plain (if we adopt a realist position) that the young organism is developing its ability to recognize, under varying conditions, properties which the object actually does possess throughout the period in question, despite what naively appear to be changes in them. (One might contend that unless the organism had some *native* ability to recognize these constancies there would be no possibility of its learning to perceive them. If it were able to bring together the various appearances belonging to identical objects, as Stagner suggests, then it must *already* have been able to recognize each of these allegedly protean objects. Not only would there then be no need for the organism to develop those specific object constancies of shape, size, and color, but also no reason why it should not assume that the objects simply did continually change their shape, size, and color. Where would it get any other hypothesis?)

However, when Stagner contends that the demand for perceptual constancies is transferred to the perception of people, he apparently means not only that their *constant* features are perceived as being constant, but that features which they actually possess only intermittently are also to be perceived as being constant. Thus he says: "If I observe Mr. Smith behaving in a weak, futile, ineffectual manner today, I shall be predisposed to observe those same characteristics in his actions tomorrow (the so-called 'halo' effect). This phenomenon is likewise adaptive and homeostatic in character. . . . Since the constancy hypothesis proves useful in dealing with inanimate objects, it tends to be transferred to dealing with people" (7, p. 8).

Note that the perception of objects as having constant shapes, sizes, and colors has now become the adoption of an all-inclusive "constancy hypothesis," referring to all their properties whatever. This is one of the many ways in which Stagner ignores the *content* of constancy. As far as those original object constancies are concerned, it is not having a *constant* perception but a *correct* one that is important, but Stagner is intent on constancy whether or no.

This "constancy hypothesis" then is held to be useful in dealing with people. Thus, he says: ". . . successful adaptation of the child to demands of his parents is unquestionably facilitated if he behaves on a constancy hypothesis with respect to them. When reality constantly frustrates this tendency (very inconsistent real behavior by a parent), maladjustment seems invariably to result" (7, p. 8). That is, in contradiction to his preceding sentence, the "constancy hypothesis" would *not* facilitate adaptation. Of course, the point is that Stagner fails to distinguish between in-

consistency and variability. Once we see that there is no inconsistency in the sense of illogicality, we realize that the parent's behavior, no matter how much it may change from day to day, always exhibits regularities. Reality, then, cannot frustrate the tendency to "perceptual constancy" in the sense that it could make the discovery of regularities—whether these be the possession of permanent predicates or the lawful recurrence of intermittent ones—intrinsically impossible. Of course, the parent's behavior may sometimes vary in a way which the child is not yet able to understand, but I suggest that it is not such failure to understand that leads to "maladjustment." The psychoanalytic view that such variability really arises from ambivalent feelings towards the child, and that it is the child's recognition of the underlying hostility which leads to "maladjustment" or anxiety, seems to me to be much more plausible and fruitful. Be that as it may, the question is always not whether something is constant, but what feature it is that it constantly exhibits.

Stagner does appear to recognize the qualitative content of constancy when he speaks of "constancies," i.e., in the plural: ". . . the organism is constantly faced with the necessity for maintaining a variety of constant states" (7, p. 10). But one might ask here why bother to use the term "constant" at all? *Any* state is constant for the time it endures, and if "constant" is here supposed to mean anything more than that, if the states really *were* "constant," then there would be no need for "the organism" to maintain them at all. However, the plurality and the qualitative nature of these constancies soon disappear when Stagner finds a common denominator for them, a general equilibrium. "The facts indicate," he says (7, p. 11), "that the epithet 'coward' may be more disturbing to equilibrium than the physical danger of facing a wild animal." But what are the forces that are *in* equilibrium? What is the resultant condition of their equilibration? One must recognize an inexhaustible (and irreducible) variety of equilibria in any organism, and while one might conceivably compare the magnitude of a disturbance in one with that of a disturbance in another, still these disturbances are distinct from one another; they are disturbances of qualitatively diverse states, and qualitatively diverse conditions result from them.

The plurality of opposing forces in the organism is finally obscured, paradoxically enough, when Stagner comes to deal with the question of choice. He says that this need for maintaining a variety of constant states faces the organism with "the task of determining priorities. . . . It should be clear . . . that the organism in some way evolves standards of value, in terms of which choice is made as to the particular constancy which gets priority" (7, p. 11).

Admittedly, it is especially easy to evade searching for the mechanisms involved when we are examining psychological or mental homeostasis, since our knowledge of even the most general nature of mental structures is negligible.

Some system of tensions, however, seems to be the general structure of mind, where the tensions are of a variety of kinds, or better, where there is a variety of structures, each becoming tense. There may be outlets (activities) which release in varying degrees the tension of more than one structure, and any one body of tension will have a variety of outlets, so that when one is blocked, e.g., by "conscience," then the tension rises until it flows through another (or perhaps by accumulation becomes strong enough to burst through the first barrier). But the point is that, because of their qualitative diversity,

the releasing of a tension is not a diminution of some undifferentiated body of energy of which all activities are a manifestation, but is always the releasing of one *specific* tension, leaving others quite untouched.

Now, what makes them a "system" is that, usually because of social pressures, the releasing of one tension can throw up obstructions which prevent the release of another. The intricacy of any given moral code is an indication of the intricacy of the web of contingencies that can grow up between various impulses. (The word "impulse" is used to indicate a structure having tension. Even though a structure may never be devoid of tension, still those two features of an "impulse" are distinguishable.) When we say we are in a state of indecision (as Stagner puts it, "the organism is faced with the task of determining priorities"), what is really happening is that two impulses or groups of impulses of approximately equal strength have come into conflict, and each is enlisting still further impulses on its side. The main means of effecting such recruitment is presumably the gaining of knowledge, or rather the acquiring of beliefs. One might put it that a given impulse *X* comes to believe, or is, as it were, assured by impulse *Y*, that the "satisfaction" (release of tension) of *X* is facilitated by the satisfaction of *Y* and opposed by the satisfaction of *Y's* opponent, *Z*. "Choice," then, is just the fact of one group of impulses simply becoming stronger than the other and forcing its way into action past that other group's obstruction.

Some such view is essential to getting rid of the mystification of Reason or the Will, the "ghost," as Gilbert Ryle calls it, and the Organism, as Stagner calls it. One cannot explain why a man "makes up his mind" one way or the other by attributing the decision to some single executive, since precisely the same problem breaks out afresh when we ask why that executive went one way rather than the other. Once again, in explaining activity we have to look for the action of the parts of the active thing on one another, and in general we might say that anything which can throw its weight into and alter the course of a conflict of impulses thereby becomes simply another coordinate impulse.

Of these, Stagner actually recognizes only the physiological processes, the "tendency to tissue constancy." Where the person seems to have a conflict between psychological and physiological impulses he is really just making a kind of mistake. Thus, in dealing with "the artist who starves in a garret to express his ideas, the martyr who prefers physical pain to an abandonment of his beliefs," Stagner suggests that this depends on "the evolution of a new perceptual object, the self or ego, and the establishment of a perceptual constancy with regard to it. . . . The tendency towards perceptual constancy will operate to hold constant the self-image" (7, p. 12). But what we really want to know is what holds constant the *self*, why does the artist go on being an artist? Stagner's theory can at most only tell us why the artist goes on *thinking* of himself as an artist, i.e., because the tendency to perceptual constancy, which in the first place was only a means to the goal of tissue constancy, has gained some sort of autonomy, has gone astray, and no longer contributes to that original goal. This intellectualism, in which the having of a certain notion of oneself is taken as sufficient to lead one toward being that sort of person, crops up again and again throughout the article. Stagner does say later that the self-image becomes so influential because the maintenance of a given ego status is important for "the maintenance of all needed equilibria," and so the self-image

is somehow "reinforced by" different "inner tensions." But if this is so, then any tendency to perceptual constancy as such is quite superfluous; the specific tensions are sufficient. Furthermore, what is this ego of which there is an image, and what *has* the image? The ego is simply that system of "inner tensions" themselves, and the clinging to a self-image is simply these various tensions being aware of each other as allies or as opponents. One never has an image of oneself as a totality, which one then seeks to attain or maintain. It is always that certain specific features are approved, and others disapproved. And the question arises again, what approves and disapproves? Not "the ghost," not some transcendent ego, but merely various component impulses struggling for outlet, and both expressing and furthering that struggle with these feelings of approval and condemnation.

The play of impulses in the mind might well be likened to the struggle of political parties, religious institutions, industrial factions, and the like in a society—a field in which the guiding hand of homeostasis has also been discovered, e.g., by Cannon himself (2) and by Dempsey (3). It seems fairly plain that such measures as the regulation of production in order to dampen the cycle of boom and depression, the granting of award wages, the legal freedom of worship are not adopted by the community for the good of all, but are produced by the fact that each of the various social groups is fanatically pursuing its own interest, intriguing, forming and dissolving alliances, calling strikes and lockouts, and being forced into grudging and temporary compromises when it is unable to muster enough strength to win a clear victory. The rough stability of a society over a period of time is not the result of the society's striving to retain its identity, or even its "acquiring its own momentum," but is just a by-product of the many-sided clash of sectional interests. There is nothing inexorable about their always balancing each other: some struggles are won and lost; societies do change in various respects and keep on changing and sometimes collapse and disappear; and so the clashing impulses in individual men sometimes balance each other and sometimes do not; men do change as their lives progress, stop being artists, embrace religion and lose faith, abandon their neuroses and commit suicide.

The doctrine of homeostasis at best only points to the facts of opposition and cooperation without advancing knowledge of the impulses whose activities these are; at worst, it hinders that inquiry by ignoring those impulses and concentrating on their resultant (or even on their mere equilibrium), and by offering a pseudosolution of how the more or less stable resultants are maintained.

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THE BRAIN ANALOGY: TRANSFER OF DIFFERENTIATION [1]

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A previous paper (3) introduced the association tracts (AT's) and dealt with three examples which involved a single US. The present Brain Analogy (BA) paper is based on a phenomenon, transfer of differentiation, which appears to involve two separate US.

Fundamentally, the BA is a circuit arrangement which has a potential connection from every sensory receptor to every motor cell. The potential connections, known as delta cells, and the sensory receptors have the magnitudes of their various properties distributed throughout the populations of delta cells and receptors in an approximately normal manner. Stimuli are converted into pulses in both the sensory and motor cells. It is approximately correct to say that the coincidence of a sensory and motor pulse at the delta cell's connection to the motor cell is the event which converts the potential connection into a functional one; subsequent stimulation of the appropriate sensory receptor causes the attached delta cell to evoke a CR from the motor cell (1, postulates). Also, there is a property of the delta cells, called the disabling function, which generally limits the number of simultaneously-active functional connections to one on any motor cell. These functional or conditioned connections are occasionally redundant, but the first conditioned delta cell into action disables the others on the same motor cell in so far as the effect of reinforcement is concerned. As a result of this property, stable conditioned connections can exist only if the action of the delta cell is initiated prior to the action of the motor cell and is continued until the action of the two cells overlaps. Backward conditioning can have but a transient existence because there are always random S, some of which are in action prior to motor cell activity, that gain control of the motor cell and disable more latent delta cells. We see here part of the competition between delta cells which forms the basis of stimulus differentiation in an appropriate experimental program; the other factor is selective extinction. Continued or repeated action of a conditioned delta cell without stimulation of the receptors of the contiguous motor cell causes extinction—returns the delta cell to its original potential-connection status.

The distribution of the magnitudes of the properties of the sensory system is the responsible agent in the automatic creation of significant or rational functional connections between the sensory and motor systems. The delta cells that are actuated at the correct time are eligible to compete in the appropriation of the motor cells and capable of holding them against the encroachments of all other delta cells. The mechanism operates on a probability basis so that if a signal is followed by reinforcement the components of the signal actuate populations of delta cells in proportion to the magnitudes of the components, and the motor cells are appropriated in the same proportions; later application of the signal evokes the appropriate CR. If a component of the signal is applied separately it will be found that the evoked CR is roughly

[1] The author wishes to express his gratitude to Vera Jane Coburn for making the drawings and for assistance in preparing the manuscript.

proportional to the magnitude of the component and is the result of the activity of the particular motor cells appropriated by that component.

The AT's were introduced (3, first two sections) to cope with more complex phenomena than can be explained by simple sensory-motor (SM) conditioning. The AT's are merely additional branches of the sensory cells which lead to other sensory cell bodies where connections are effected by means of delta cells in a manner exactly the same as with connections to motor cells. As a result of these auxiliary connections it becomes possible to establish conditioned connections from one sensory function to another as in sensory preconditioning. With the AT hypothesis it was found desirable to limit generalized connections to the AT's, and hence all generalized CR's are effected from generalized sensory neurons through specific sensory neurons to motor neurons.

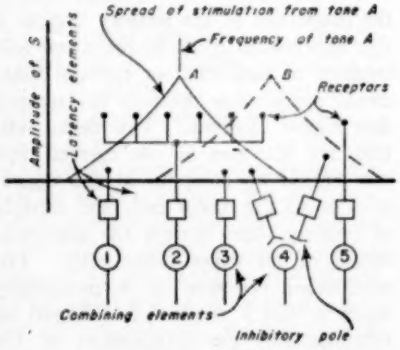

FIG. 1. The spread of auditory stimulation is possibly due, in part at least, to the fact that the basilar membrane deflects and stimulates receptors embedded in areas adjacent to the "point" of stimulation. This "generalization" is probably of insufficient degree to account for the facts without support from nonperipheral mechanisms. The intent in these figures is to simulate function rather than structure.

In this paper generalization is assumed sufficient to permit any overt S to act as a substitute for another. If we are differentiating two auditory S, say, it is probable that certain receptors will be actuated by each S unless they are low in strength and far apart in frequency. Suppose two tones, *A* and *B*, are of equal strength and less than an octave apart. Referring to Fig. 1 we see that, to the nervous system, the sphere of influence of each tone overlaps that of the other (4, p. 44).

The symbols used in the figures are the same as before (1). Briefly, the plus sign indicates a reinforced trial; minus, nonreinforced; zero, nonactuated; a parenthesis indicates an occasional trial; *S* and *L* equal short and long latencies, respectively.

TRANSFER OF DIFFERENTIATION

Pavlov's experiments on transfer of differentiation (5, p. 228) are concerned with two salivary functions. If these are entirely independent US, as implied by Pavlov, there is no ambiguity from that source in the following discussion. The experimental program consists in establishing tone *A*, say, as the CS for food and then differentiating tone *C* which has also become conditioned because of both generalization and closeness of physical relationship (4, p. 44). When the differentiation is established, *A* is given a series of trials in which it is followed by acid instead of food; this presumably causes the original salivary CR to become extinct and another qualitatively different salivary reflex becomes established. Upon testing again with *C* it is found that it does not elicit any response; the differentiation has been "transferred" to a new US.

Pavlov demonstrated that a new and finer differentiation could then be established by applying an intermediate tone, *B*, occasionally, and not reinforcing with

acid. This new differentiation was found to be transferable back to the original US, food.

Our S situation corresponds with Fig. 2 because overt *A*, *B*, and *C* are auditory S of less than an octave apart. None is ever actuated with another, *A* is always followed by food (or later, acid), and overt *B* and *C* are never overtly reinforced.

Figure 3 shows that S differentiation takes place partly at the AT level rather than at the SM level as indicated earlier (1, p. 173). Overt *A* actuates *1*, *2*, *7*, *8*, *9*, *10*, and *13*. Neurons *2*, *8*, and *10*, having long latency, are therefore objects of competition by neurons *1*, *7*, *9*, and *13*. The generalized group, *13*, is not a small population and no amount of application of overt *A* will cause differentiation since *1*, *7*, *9*, and *13* are components of a compound S as viewed by *2*, *8*, and *10*. Yet differentiation is always available by rendering, say, *9* and *13* unstable—by applying occasionally overt tone *C* which stimulates *9* and *13* (among others) but not *1* or *7*; reinforcement by US_1 is omitted. Neurons *1* and *7*, which represent only the shorter latencies, become exclusive CS to neurons *2* and *8*, while *9* and the generalized connection, *13*, gradually become extinct at neurons *2* and *8* because of occasional nonreinforced (absence of *A*) trials.

In addition to *9* and *13*, *C* stimulates *5*, *6*, *10*, *11*, and *12*. Only *10* concerns us now—the others will be disposed of later. Before any differentiation was effected, the group represented by *10* was appropriated by *1*, *7*, *9*, and *13* as indicated above. Differentiation, as a process, did not eliminate *9* and *13* from sharing control of *10* because the latter was also stimulated by *C* and therefore reinforced the AT connections that were actuated. Those portions of group *10* which were originally appropriated by

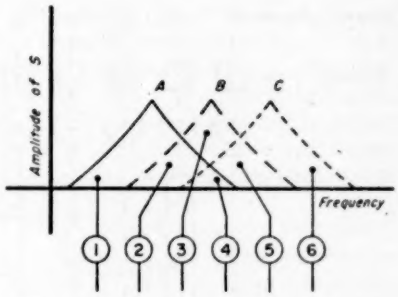

FIG. 2. Receptor properties, in effect, are partly a function of the magnitude and location in the sensory field of various stimuli and combinations of stimuli. For the particular program shown here, neuron *4* is generalized since *A*, *B*, or *C* will actuate it. Neurons *2* and *5*, on the other hand, are sensitive to only two of the stimuli. It is apparent that a reduction in magnitude of *A* and *C* will have the effect of changing *4* from a generalized to a specialized neuron. This dependence of properties on the experimental program is of considerable significance in altering the relative populations of delta cells available to the overt stimuli; the consequence is sometimes a marked difference in behavior.

1 and *7* were not disabled, since *1* and *7* are not actuated by *C*, and are therefore available for *additional* conditioned connections from any actuated, short latency, AT's: *5*, *9*, *11*, and *13*. Connections from *5* and *11* are supernumerary [2] with respect to applied *A* but *9* and *13* are redundant and probably most are extinctive since *1* and *7* were established competitively in step *1*. At the conclusion of differentiation the part of group *10* originally held by *1* and *7* is also held simultaneously by *5* and *11* in

[2] It is necessary to distinguish between redundant and supernumerary conditioning. Both are characterized by the presence of two or more conditioned connections to one sensory or motor cell body, but the conditioning is described as redundant only when two or more conditioned connections are in action simultaneously, otherwise it is supernumerary. In the latter case the connections are entirely without effect on one another in so far as reinforcement and the disabling function are concerned.

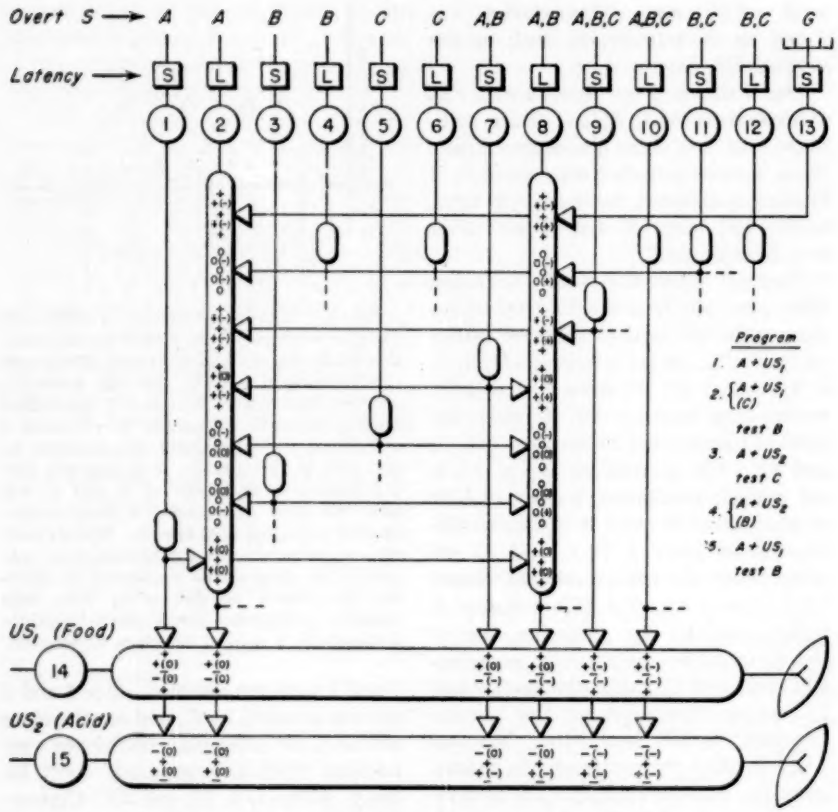
Overt S → A A B B C C A,B A,B A,B,C A,B,C B,C B,C G
Latency →
S L S L S L S L S L S L S
1 2 3 4 5 6 7 8 9 10 11 12 13
Program
1. A + US1
2. A + US1 (C)
test B
3. A + US2
test C
4. A + US2 (B)
5. A + US1
test B
US1 (Food)
14
US2 (Acid)
15

FIG. 3. This figure is necessarily a condensed version of the theory of transfer of S differentiation and can be interpreted properly only in conjunction with the argument in the text. The conditioning symbols should be read down the columns, each horizontal line corresponding to a numbered step in the program; no symbols are shown for the test trials interspersed between formal program steps.

four combinations. This is a failure of differentiation at the AT level. It will be seen, though, that differentiation drives to extinction the SM connections from *10* to *14* because *10* is actuated by any S and reinforcement by US_1 is not inevitable. SM connections from *1*, *2*, *7*, and *8*, not actuated when US_1 is absent, gradually gain exclusive control of *14*—no overt response follows application of overt *C*.

Thus far we have covered only the first two steps in the experimental program. Before proceeding further some digressions are necessary.

Allusion has been made before to the fact that AT conditioning is without significant effect unless the program is altered. A related property is the fact that sensory cells, as a group, must be excited directly rather than through AT connections in order to form relatively stable, numerically significant SM connections. The phenomenon does not seem to approach a limit, like habit saturation, and therefore a quantitative

approach is necessary. But this is impossible until suitable parameters have been established; for the present we place another burden on intuition.

Consider two sensory neurons, *X* and *Y*, and one motor neuron, *Z*. The program consists of trials of *X* and *Y* together, interspersed with trials of *Y* and *Z* together. During the former trials an AT connection develops from *Y* to *X;* during the latter trials an SM connection is made from *Y* to *Z*, or, if the AT connection is functional, chance will determine whether *X* or *Y* becomes conditioned. Actually, the AT delta cell and the *Y*-group SM delta cell do not have to be, and usually would not be, parts of the same neuron. Some other neuron, *Y'*, for example, could gain control at the SM level. In any event, the AT and SM connections are subjected to an interspersed set of reinforced and nonreinforced trials: when the US is present the AT connection is not reinforced by *X* and when the US is absent the SM connections are driven toward extinction regardless of whether the sensory neuron considered is excited directly or through an AT connection. The circumstances of the assumed program are such that a group of *X* cells and a group of *Y* cells are in competition for control of *Z*, even though *X* cells are never excited directly when *Z* is actuated. The effective degree of competition is dependent on the nature of the program.

It will be observed that an *X* cell must be more latent (or be actuated later) than the *Y* cell which acts as a CS through the AT's. But this has no effect at the SM level because relative latencies are without influence, once a delta cell becomes conditioned, provided its action is initiated prior to that of the motor cell. The first *conditioned* delta cell into action disables all other delta cells on the same cell body (1, Postulate 13).

The circuit from *Y* through *X* to *Z* has less stability than that from *Y* to *Z*, directly, because of the two delta cells in series. The probability of encountering a short gamma phase cell is greater in the series path by a factor greater than one and less than two compared to the direct path.

The maximum *equality* of stability occurs when *X* and *Y* are usually actuated and the *Y* plus *Z* pair is seldom actuated. But this is the case when SM conditioning is nearly impossible to establish. The minimum equality of stability occurs when the above frequencies are reversed. In this case *Y* almost completely excludes *X* from *Z*. When the trials alternate, the *Y* connection to *Z* has roughly twice the stability of the *X* connection to *Z* because of the gamma phase distribution.

The typical differentiation program consists of reinforced trials interspersed with occasional nonreinforced trials. It is assumed that Pavlov's data (**5**, p. 228) correspond because no mention was made of any other procedure. With this assumption and the foregoing argument, it is seen that the neurons which are reinforced only when excited indirectly are also the ones that are excited infrequently; the resultant is a minimum equality of stability. In other words, neurons *4*, *6*, and *12*, representing overt *B* and *C*, cannot have significant access to *14* or *15* and therefore they are shown without input connections from the AT's in spite of the fact that such connections might be functional at times.

Neurons *1*, *3*, *5*, *7*, *9*, and *11* are shown without AT connections to their cell bodies because short latencies prevent stable conditioning from any other neurons in Fig. 3.

Returning again to the first step in the program, neurons *1*, *7*, *9*, and *13* are seen as components of a compound S as viewed by neuron *2*. They gain

joint control of *2* which, in the second step, becomes restricted to *1* and *7*. In the third step the latter two disable *9* and *13*, preventing renewed access. The fourth step drives the connection from *7* to extinction leaving *1* in sole possession of *2*.

Neuron *8* has a more complex history. The first three steps exactly parallel neuron *2* but the fourth step does not drive *7* to extinction because *8* is actuated by *B*. Neuron *8* then represents two portions. The first consists of those appropriated by *7* and continued in power by the program; the second consists of those appropriated by *1* which, not being actuated, cannot disable AT connections and therefore *3*, *7*, *9*, *11*, and *13* gain access to *8*. The resulting conditioning from *7*, *9*, and *13* is redundant in the fifth step but repeated application of *A*, in accordance with the program, eliminates the redundancy. The second portion of *8* is thereby consigned to neurons *1*, *7*, *9*, and *13* in proportion to their populations. Although the redundant connections are eliminated, the supernumerary ones are not; these are *3* and *11* which are silent partners of *1* in connections to *8*. It is easily seen that all or part of *8* can be actuated by overt *A*, *B*, or *C* after the fifth step in the program. With so little known concerning the mechanism that limits S differentiation, there is no recourse but to continue to appeal to the fact that differentiation *does* have limitations and it is not inconsistent to point to the relative populations as pertinent to the problem: when *A*, *B*, and *C* are too close in frequency the populations of *7*, *8*, *9*, and *10* are too large for effective transfer of differentiation.

Neuron *10* is actuated on every trial without exception. The explanation of of the resulting behavior of the AT conditioning is somewhat tedious and of little interest. It suffices to note that since *10* invariably responds it is necessarily driven to extinction in its connections to *14* and *15* if the latter two are sometimes not actuated and if any other sensory cell can acquire stable connections. These conditions are met in steps two and four, only. In the fifth step *10* aligns itself with *8* in opposing transfer of differentiation but it has a still smaller population.

The SM conditioning is relatively simple. In the first step neurons *1*, *2*, *7*, *8*, *9*, and *10* are components of a compound S as viewed by *14*. The second step eliminates the connections from *9* and *10*, transferring the unappropriated motor cells to *1*, *2*, *7*, and *8*. A test application of *C* reveals the differentiation but a test of *B* actuates *7* and *8*, and some of *2* through an AT connection from *7*, resulting in overt response of *14*. The third step drives all components to extinction on *14* and gives *1*, *2*, *7*, *8*, *9*, and *10* access to *15*. Since neurons *9* and *10* are not denied access to *15* it appears that transfer of differentiation has failed. And so it has, if *9* and *10* are large components. The magnitude of these components is roughly inversely proportional to the frequency difference between *A* and *C* with the result that the process approaches a limit as *C* approaches some value near *A* (in separate experiments). But while the SM differentiation was lost in the transfer, the AT differentiation remained untouched. Those outlets that *C* might have, by means of AT conditioning, through connections from *9* and *13* to *2* and *8* were driven to extinction before the transfer and subsequently were denied access by means of the disabling functions in *2* and *8*. As long as *9* and *10* are relatively small components transfer of differentiation is successful. As the frequency difference between overt *A* and *C* is reduced, in separate experiments, the populations of *9* and *10* increase and the ability to discriminate decreases. Finally, the

animal exhibits "defense" reactions and it is conjectured that these interfere with a smooth transition.

The fourth step in the program drives to extinction the SM conditioning from *7*, *8*, *9*, and *10* to *15* and the AT connections from overt *B* to *2* follow the same course. The fifth step breaks all connections to *15* and re-establishes connections from *1*, *2*, *7*, *8*, *9*, and *10* to *14*. But *1*, disabling all other AT connections to *2*, prevents a test application of overt *B* or *C* from causing a response through *2*. The direct connections from *7*, *8*, *9*, and *10* to *14*, as indicated earlier, oppose transfer of differentiation. If a considerable part of the response to tests is through AT connections to *2*, and these connections are broken, the response should decline perceptibly.

Since *9*, *10*, and *13* are in effect all generalized neurons, and since *9* and *10* have all the properties of *13* plus some additional properties, *13* seems superfluous. But this is decidedly not the case. The relatively large population of *13* has a significant function: it permits effective generalization through AT connections without at the same time preventing transfer of differentiation.

Transfer of differentiation is possible because of the existence of a relatively large population of generalized neurons which have no direct connection to any motor cell.

DISCUSSION

Several difficulties afflict the AT hypothesis as delineated in this and the preceding paper (3); three are mentioned here. First, it is apparent that if AT connections are made from short latency *A* neurons to longer latency *A* neurons during "primary" conditioning, *B* neurons cannot then gain access to any *A* neurons excepting those of short enough latency to avoid capture by other *A* neurons. While external access to *A* is thus severely limited, it may not be serious because of the connections internal to the *A* group.

Second, the AT's tend to reduce the latency of the overt response because a signal can avoid the delay of the long latency elements by a traverse through the short latency neurons and their AT's. The situation here is similar to the general case of a delayed CR. The reader can easily determine for himself that monopolar neurons are not adequate for delayed reflexes because all neurons being actuated for the same length of time regardless of latency, and long enough to overlap the US, are also subject to decrement for equal intervals. Those neurons possessing a long enough gamma phase gain control of the motor cells, but there is no differential action where latency is concerned and, therefore, the CR after training is actuated gradually and without any definite delay period. A nonresponsive delay period is assured by the simple expedient of excitatory bipolars which have two latencies individually subject to the usual distribution function. Those with a short overlap of paired latencies which is appropriately timed for the particular delay program are actuated briefly and only after considerable delay, hence they do not deplete the gamma phase and do not excite the response too soon; they are differentially favored on a latency basis in the struggle for control of the motor cells. It seems possible that the same mechanism may be applied to the AT's to prevent premature response.

Third, there are transient phenomena with which to cope. In Fig. 3, neuron *14*, it is shown symbolically that the connections from *1*, *2*, *7*, and *8* are occasionally not actuated on the second step of the program. In this case, and similarly for Fig. 4 of the preceding paper (3), the nonactuated stage is not reached until after the AT connections are broken, and therefore a transient

nonreinforced condition exists. However, the symbols correctly indicate the status after the initial transient. In this connection it is again worth noting that while *single* occasional nonreinforced trials drive some delta cells to extinction, many have a sufficiently long gamma phase to be unaffected; therefore, the habit is given an increment of immunity to single nonreinforced trials. To avoid this obstruction to differentiation it is necessary to apply several successive nonreinforced trials (1, p. 170).

COMPLEX GENERALIZED NEURONS

Another paper (3) mentioned the need for more complex structures to cope with both laboratory and "natural" environments and gave a definition of a monopolar, generalized neuron suitable for use in elementary environments. Since the requirements for bipolar generalized neurons have not been studied thoroughly, no definition is offered.

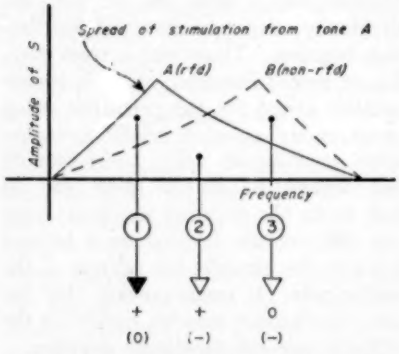

FIG. 4. When all the facts are not known, a number of possible explanations can exist for the same phenomenon. It is postulated in this figure that any auditory stimulus excites the entire auditory field. The variation in magnitude of stimulation serves to classify the neurons in a functional manner not contemplated in their structures. Neuron *2* is generalized and driven to extinction by the method of contrasts though it has precisely the same internal properties as neuron *1* which gains control of the response.

For the present paper it suffices to introduce the subject and indicate its significance.

Conditioning to tone *A*, say, shows generalization over most, if not all, of the audio spectrum (5, p. 113). If this result were attributed to direct stimulation of the entire basilar membrane, and if only monopolars are postulated, it is still possible to apply any tone without getting a response from all auditory neurons because of the gradient of stimulation and the distribution of thresholds as shown in Fig. 4. Then it is also possible to effect differentiation, because some neurons that are actuated on reinforced trials receive no stimulation on nonreinforced trials—these are stable and gain exclusive control of the motor cells in a laboratory environment. Galambos and Davis (4, p. 44) have shown that *receptors* are not excited by all tones, even though the receptors have low thresholds; but this does not rule out the possibility that "point" stimulation spreads to other neurons at subsequent stages in the nervous system and therefore acts like direct stimulation of the entire basilar membrane.

Neuron *2*, in Fig. 4, though structurally identical with the others, because of its location and threshold acts like a generalized neuron. In a natural environment it is rapidly driven to extinction—differentiation is effected—and *1*, having a somewhat higher threshold, survives longer but ultimately shares the same fate.

At least the gross aspects of generalization and differentiation are portrayed by several possible mechanisms and the author cannot as yet show an array of evidence that elects any one of the group. The subject needs much more study, especially so in view of the probability that more than one mechanism is in use. For practical reasons the fundamental properties originally postulated

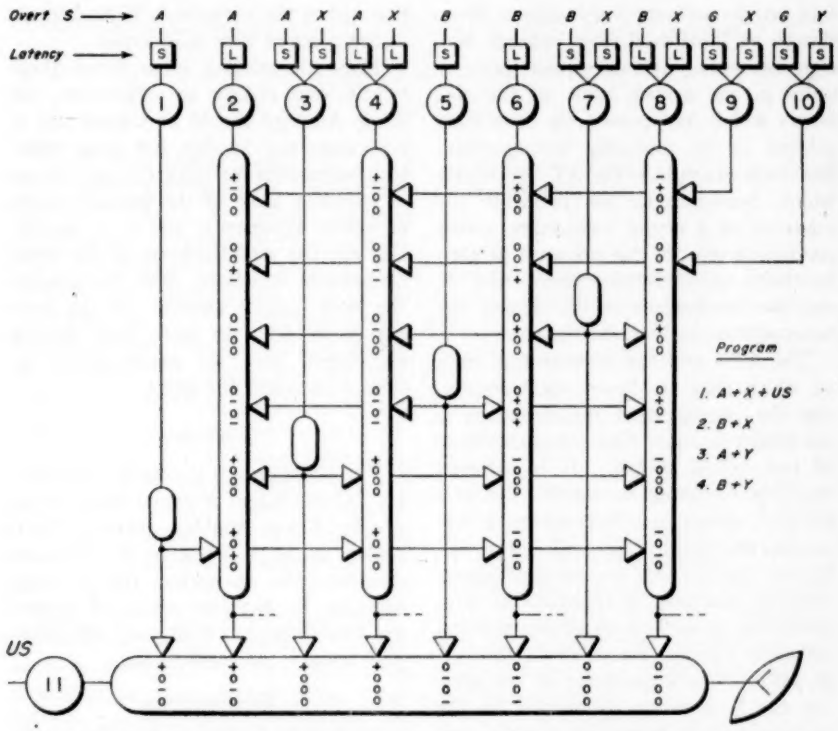

FIG. 5. The elements of the program are considered to be applied in random order. The first two represent the laboratory environment, the latter a "natural" environment. Neither the undifferentiated nor the differentiated laboratory-established CR is significantly affected by the natural environment provided the monopolar population is relatively small and the generalized neurons are functionally bipolar.

(1, p. 173) are retained and incorporated in a bipolar structure.

Figure 5 depicts the mechanism of generalization and differentiation anchored in bipolar neurons so that exposure to varied environments does not constitute cause for disagreement with fact. It will be observed that a generalized neuron now has its generalization property confined to one pole of a bipolar neuron so that its action is dependent on an additional factor in the environment. At the same time generalization, as a phenomenon, is not restricted because there are many generalized neurons with a broad range of properties to cover the action of all sensory fields. It is realized that many of these BA concepts are arbitrary, some possibly in the extreme, but they seem to be useful even now and it is hoped that by successive approximations they will in time bear a greater semblance to reality—whatever it may be.

CONCLUSION

Science displays numerous instances where mutually exclusive concepts are found to be limited in scope so that both points of view are finally merged in subsequent theories. Since the original concepts have some substantial basis in

fact, we do not expect subsequent theories to be fabricated from entirely foreign elements. We *do* expect them to unite in one system some of the elements which had previously been considered to be mutually incompatible. One such example is the AT hypothesis which demonstrates the probable coexistence of a neural mechanism which conditions only in the presence of conventional overt reinforcement and of another mechanism which denies the necessity for such reinforcement.

The AT's and the experimental data on which they are based again emphasize the concept that reinforcement is essentially no more than the coincidence of two neural events. It is believed that the tendency to consider various external agents as reinforcement is anthropomorphic in origin and is responsible for the fact that one external agent, food for example, is classified as reinforcement in various situations without sufficient concern for other factors (2, p. 458). The importance of considering the S situation *as viewed by the nervous system* cannot be overstressed.

The slight degree of genuine understanding that exists concerning the nature of intelligence is illustrated by the ease with which we can ask questions that embarrass a theory. For example: if sensory association exists, as postulated, why do we not see a flash of light after a particular S if the two had previously been associated? This question may have kept others from making a conjecture as to the nature of the mechanism. Possibly the author should follow suit, for it is only too apparent that either the hypothesis is inadequate or the point of view is incorrect.

These disquieting facts breed skepticism—and rightly so. However, the Brain Analogy should be viewed not as possessing any finality, but as an effective instrument for research, as a means of avoiding some of the inconsistencies of verbal arguments, and as a foundation for the establishment of the interchangeable constants that characterize the more mature sciences. If the present paper does no more than suggest significant lines for experimental attack, it is worth the effort.

SUMMARY

The phenomenon of transfer of stimulus differentiation is explained in terms of the Brain Analogy theory. It is shown to be a property of the same structure, the association tracts, which accounts for the phenomena of secondary conditioning, conditioned inhibition, and sensory preconditioning.

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A COMMENT ON BURKE'S ADDITIVE SCALES AND STATISTICS

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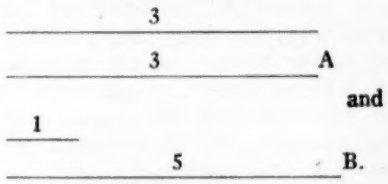
Burke, in a recent article (1), states that "statistical technique begins and ends with numbers and with statements about them." Therefore, he concludes, "the properties of a set of numbers *as a measurement scale* should have no effect upon the choice of statistical techniques for representing and interpreting the numbers." Again, referring to statistical interpretation, Burke states that "the use of the sample mean and standard deviation does no violence upon the data, *whatever the properties of the measurement scale.* Thus, the use of the usual statistical tests is limited only by the well-known statistical restrictions" (italics mine).

Some rather unfortunate implications follow from Burke's position. When numbers have been assigned to objects according to some stated rules these numbers can indeed be manipulated in any way we desire. But once the manipulations have been completed and the tests made, a dilemma arises. If we have performed operations on the numbers which we could not perform on the objects, we must choose between two interpretive procedures. We must either assume that (*a*) what is true of the numbers is also true of the objects, or (*b*) what is true of the numbers is not necessarily true of the objects.

The first assumption leads to all sorts of difficulties, which have been adequately described by Campbell (2), Stevens (4), Reese (3), and others. These difficulties may be illustrated, in oversimplified form, by two examples, one numerical and one geometric.

Suppose we have a measurement scale which has ordinal but not interval or additive properties. Such a yardstick is illustrated below:

An object is measured by laying it against this yardstick in the usual way. We are given two pairs of objects, A and B, whose lengths are:



The numbers assigned to the objects total 6 in both cases, but inspection will reveal that the additions of the objects will give a longer line in case A than in case B.

Thus the statement that *there is no difference between the sums of the numbers used to represent length in the two cases* is correct, but strongly suggests the erroneous conclusion that if the two summed lines were laid side by side, no difference could be discerned between them.

The absurdity of possible conclusions may also be revealed by a numerical example. Suppose families are grouped according to income on a scale where the number 5 means "very rich" and number 1, "very poor." The actual income intervals, however, are unequal, as follows:

Number	*Income limits*		*Midpoint*
5	$5,000–	$1,000,000	$502,500
4	3,000–	5,000	4,000
3	2,000–	3,000	2,500
2	1,000–	2,000	1,500
1	0–	1,000	500

In town A all the families have incomes which fall in class 3, while in town B half the families have incomes in class 5, and half, incomes in class 1. Both towns will have a *mean* of 3 if we consider only the numbers assigned to the categories, but it is evident that town B, with a mean income of $251,000 is richer than town A with a mean income of $2,500.

If we accept conclusion *b*, on the other hand, we are in an even more ridiculous position. Though our statistical procedures may have been perfectly justified and our interpretations correct when considered strictly in relation to the *numbers*, we can make no interpretation about the properties of the objects to which the numbers have been assigned. As psychologists, we can draw no conclusions about responses, organisms, or behavior, but only about numbers.

Since psychologists are presumably more interested in the behavior they describe with numbers than in the numbers themselves, they will learn more if their statistical techniques correspond with the properties of the set of numbers as a measurement scale than if these properties "have no effect upon the choice of statistical techniques for representing and interpreting the numbers."

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